

24 April 1980

Are books too expensive?

We love books not merely for what's in them. Books feel good, even smell good. Touching them, we rub the grain of their bindings, we skate our fingers across fine paper, we run our thumbs through pages as if they were guitar strings. They feel good whether they are words by Flaubert or encyclopaedias of pharmacology.

In a bookshop, we can rest our arm on a shelf lost in the art of Chinese porcelains as we face a stack of paperback jackets glowing with sex. We may dash into the shop to get out of the rain, and yet walk out with a storm of ideas, provoked by a chapter heading.

Until quite recently, perhaps our only regret, stimulated by an encounter with books in a bookshop, was the certainty that one life could never give us enough time to read all the books worth reading. There once was a time we could pay our bill and walk out into the street with a few titles under our arm, all for the price of the lunch skipped that afternoon.

Today, of course, the prices confound the pleasures of buying books. What we once could purchase for the price of a skipped lunch, now costs us the equivalent of a dinner in a first-class restaurant. And when we get the bill for scholarly books the tally looks like what we might have expected to spend for a meal in a great French restaurant.

Book reviewers for this periodical often complain about the high prices. Young scientists despair at ever being able to afford them. And the rest of us grumble, often attributing the prices to the greed of publishers. What is to be done? Some argue that if publishers of scientific and technical books showed some restraint, restricting the number of symposia or limiting the output of House titles which are of interest only to narrow specialists, then prices would fall. According to this claim the prices of scholarly books are inversely proportional to the population served. Consequently, publishers must choose books for larger audiences.

There are those who suggest that the best way of lowering the prices of scholarly books is to eliminate unnecessary duplication of titles. Take, for example, the book displays at the Federation of American Societies for Experimental Biology meeting in California a week ago, where more than two dozen publishers exhibited their books. Biologists interested in a book on, let us say, interferon, could choose from among five or six new volumes on the subject, each one from a different publisher along the aisle. We would turn fewer trees to pulp and prices would fall if only a single publisher issued a book on each topic, or so this argument goes.

The difficulty with all these solutions is that the regulation of the science book publishing industry would be much like regulating science itself. Scientific literature rides the caboose on the train of science. Where science goes, publishers inevitably follow.

In order to make any of these proposals work, we will require men and women of great vision. They will need, for example, to choose from among the hundreds of symposia held each year those which should be privileged to appear in print. They will need to determine which are destined to be the classics, to be used in classrooms and laboratories over the next decade, and which should end with the after-dinner speech at the closing banquet.

Or consider those books which might never have been published had someone thought them too specialized. Who among us has the power to predict which area of investigation, now at the periphery of science, will some day emerge at the core?

And as for the proliferation of similar titles on the same subject: can any of us pick the author who will write the best book?

It is true that the rain of books falls most heavily on the libraries. With each slash of the budget, librarians can afford to buy fewer books. As libraries, traditionally the principal customers for scholarly publications, purchase still fewer titles, publishers are forced to charge yet higher prices. And so with each twist of the inflationary spiral, libraries and publishers alike struggle to breathe.

Yet neither inflation nor the ineluctable way in which science unfolds offers absolution to the publishers. We all recognize those who maintain their dignity, even under the strain of conflicting market forces, by their good books. There are those publishers, however, who acknowledge that in order to feed their hungry publishing mill, they must grind out lots of books — good or bad, it hardly makes any difference. Some publishers don't even blush when they compare their own books with sausages or shoelaces.

When will it all end? When will we be able to walk into our local bookshop again, pull a volume off the shelf, and pay the bill without anxiety? For the moment the outlook is grim and will remain so at least until the pace of worldwide inflation slackens. Publishers are not immune to the rapid rise in costs experienced nearly everywhere. Paper, binding, typesetting, distribution and advertising costs are likely to scramble to still higher peaks as costs climb generally.

When we learn how to control an economy gone berserk, we may learn how to control the prices of books. □



United States

Threat to marine science lessens

The new draft of the Law of the Sea Treaty has eased potential restrictions on international marine research. But not all nations are entirely happy with the result. **David Dickson** reports from New York

PRESSURE coming largely from the US scientific community has resulted in a number of changes in the proposed Law of the Sea Treaty, under debate in the United Nations since the early 1970s. The changes are aimed at easing potential restrictions on the conduct of scientific research in regions of the oceans directly controlled by coastal states.

Few of those involved in the negotiations are entirely happy with the outcome. Many nations with large off-shore resource deposits — in particular Canada and some of the Latin American states — would have preferred to stay with the strict controls over research initially proposed, while the scientists of other nations would have liked greater movement in the direction of free scientific enquiry.

Both admit, however, that some compromise between the goals of scientific inquiry and the political and commercial demands of resource management has become necessary. And the changes which have been introduced into a new draft of the treaty, approved at the end of a negotiating session in New York earlier this month, will go to Geneva in the summer for what is hoped to be the final meeting before the treaty is agreed and signed early next year.

Earlier drafts proposed giving coastal states almost absolute authority over research carried out in waters under their control had brought strong protests from scientists who claimed that this would be a direct restriction to the freedom of scientific inquiry.

One of the strongest criticisms came from the US National Academy of Sciences, whose ocean policy committee issued a statement in 1977 warning that unless changes were made in the draft text, the proposed treaty would "cripple future marine scientific research" critical to the survival of the oceans and of mankind.

Since then, the treaty's provisions have been modified as negotiators have attempted to tread a delicate path between commercial and scientific interests, the two connected by the fact that information about resource deposits — for example oil and natural gas — can have important commercial implications when licensing or exploration rights are being considered.

At an early stage, for example, the US dropped its demands that one guiding principle should be the "obligations" of nations to provide for the interests of scientific researchers. It has now been accepted that research within a 200-mile

economic zone and on the continental shelf can only be carried out with the consent of the coastal state, but with the understanding that such consent must be granted when the research project is conducted for peaceful purposes and fulfills other specified criteria.

There have also been modifications to the rules governing the publication of research findings. Previously these would have been strictly controlled by the coastal states involved, triggering complaints of potential censorship. Now the conditions under which permission to publish can be refused have been tightly specified, and a researcher must be told of these conditions before his project begins.

Tentative agreement on the three main outstanding issues of interpretation was reached during the New York negotiations, based largely on proposals put to a working committee by its chairman Dr Alexandrov Yankov of Bulgaria, which the US delegation had already indicated it was prepared to support.

Firstly, there was agreement that disputes over the withholding of consent should be referred to compulsory conciliation, where previously there had been no scope for such a decision to be appealed.

There was also agreement that if a coastal state considers that research to which it has given its consent is carried out in a way that violates the treaty, it now has to give the research worker time to amend the procedures, and no longer has the power summarily to terminate the research project.

Finally, the new draft contains revised wording about the conditions under which a coastal state with a wide continental shelf can refuse permission for research in areas of the shelf outside the 200-mile limit otherwise agreed as the limit of economic responsibility.

Here the wording finally agreed was that permission to carry out research could only be refused in situations where detailed exploitation or exploratory operations were already occurring, or were about to occur within a reasonable period of time.

Negotiators in New York spent considerable time over the wording of this paragraph, since ambiguity over the meaning of "detailed" exploration left a number of countries unhappy that, unless test drills were virtually in place, they could not deny scientists access to areas in which commercially valuable deposits might lie.

Indeed, in the final plenary session of the New York meeting the Canadian delegate

argued that the conference might be building "potentially serious interpretation problems" into the proposed treaty with such wording, adding that "my delegation would have much preferred a solution with more specific concrete provisions clearly affirming the rights of coastal states relating to the conduct of marine scientific research on the continental shelf beyond 200 miles."

But US scientists who, with colleagues from the Federal Republic of Germany, have been pushing strongly to ease the initial restrictions, feel that the changes have made the treaty, if not better, at least less unacceptable than previously.

"We still do not feel that the deal on scientific research is as good as it should be," says Dr John Knauss, Dean of Oceanography at the University of Rhode Island. But he adds: "It's now a question of whether we have a treaty with these amendments or no treaty at all". Others feel that, although the scientific community is unlikely to back the treaty actively, it is also unlikely to oppose it in its present form.

Despite agreement in New York on almost 90% of the proposed text for the Final Law of the Sea, some major problems remain before the treaty can be signed. One of the largest is the composition of the council for the International Sea-Bed Authority responsible for allocating mining rights and determining the conditions under which resources can be exploited.

Here the developed countries, keen that the activities of their industries should experience minimal restrictions, are urging that only five of the council's 35 members should be able to prevent a mining contract from being refused — in other words, should have power to grant a contract. (Five of the western developed countries — US, Japan, West Germany, France and the UK — would have seats on the council.)

In contrast, the developing countries are arguing that it would be more democratic for nine votes to be required for a contract to be awarded, and that a similar number would also be sufficient to block the award of a contract, numbers which the western countries feel could throw them in to an uncomfortable alliance with the Soviet-bloc.

Despite such disagreement, UN officials still hope that sufficient progress has been made to conclude the final version of the treaty during the Geneva meeting, and for it to be signed next spring. The next problem will be getting it accepted. And in the US this means convincing the Senate, responsible for ratifying all foreign treaties, that the changes have been sufficient to meet the previously expressed concerns. □

US/UK collaboration proposed for breeder technology study

A US congressional committee has approved funds for a joint US/UK study of the future development of liquid metal fast breeder reactor (LMFBR) technology, intended as part of the current programme to build such a reactor at Clinch River in Tennessee.

Precise details on how the money would be spent have yet to be determined. And whether the joint study manages to get off the ground depends largely on how much support can be generated for it in Congress and the administration.

However if the funding is approved the study would, according to US officials, provide a way of formalising the existing interchange of information between nuclear scientists in the two countries, perhaps leading to agreement on efforts to avoid duplication and to help in filling gaps in each other's programmes.

The joint study has been proposed by Energy Production Subcommittee of the House Committee on Science and Technology, which has consistently — and so far successfully — opposed President Carter's attempts since 1977 to terminate the Clinch River project.

Disagreeing with the President that fast breeder development should be delayed either because of the increased risks of nuclear proliferation or because of declining forecasts of energy demands, the subcommittee last year recommended \$183 million be spent in 1980 on the project, which had been dropped entirely from the Department of Energy's budget request.



Clinch River: technical team to cooperate with UK scientists.

Congress subsequently approved spending \$172 million on the project.

This year, after the President had again recommended terminating the project in the budget for 1981, the subcommittee is proposing a budget of \$155 million. And of this, "up to \$15 million" would be for a joint US/UK study with particular emphasis on future fast breeder reactors.

Introducing an amendment to the DOE's budget request, Mrs Marilyn Lloyd Bouquard of Tennessee said that such an effort would be in line with the recommendations made in the final report of the International Nuclear Fuel Cycle Evaluation (INFCE). Ultimately, she said, it would lower the cost of fast breeders.

According to Mrs Bouquard, the proposed joint study would as far as

possible draw upon the existing technical team, design information and data base of the Clinch River project, whose design is virtually finished but for which the DOE has not requested a construction permit.

A spokesman for the United Kingdom Atomic Energy Authority said that already agreement to exchange facilities with the US on fast breeder technology had been reached in September 1979: the UK will subject fuel assemblies to "gross abuses" in the US 'TREAT' test facility, while US researchers will have some access to the prototype fast reactor at Dounreay. But negotiations with the US, and France and Germany, for more extensive exchange on the scale envisaged by the congressional sub-committee were "at a very early stage", pending firm British commitment to a fast reactor policy. □

Panel urges research on tropics before forests disappear

A substantial acceleration of the pace of compiling biological inventories of tropical plants and animals, and a four to five-fold increase in the number of appropriately trained taxonomists to help achieve this, have been recommended in a report on tropical biology by a panel of the National Academy of Sciences. The report, shortly to be published in Washington, warns of the increasingly rapid disappearance of the world's forests.

The panel also lists a number of areas of tropical forests, including the coastal forests of Ecuador, eastern and southern Brazilian Amazonia, and western and southern Cameroon, which it says should receive particular attention by taxonomists over the next 5 to 10 years, since they contain little-known biota in immediate danger of extinction.

And it recommends that a comprehensive laboratory should be established in Puerto Rico as a centre for research and training in tropical physiological plant ecology.

"The creation of such a laboratory appears to be critical for the establishment of a field of investigation that is poorly developed in the tropics. Such investigation appears to offer a great deal

both in scientific and societal rewards" the report says.

The report was prepared by the Committee on Research Priorities in Tropical Biology of the Academy's National Research Council, chaired by Dr Peter H Raven of Missouri Botanical Gardens, St Louis, Missouri.

Speaking in Washington last week, Dr Raven pointed out that less than 10% of the Earth's estimated four to five million species have yet been properly classified, but that while scientific knowledge of tropical ecosystems was virtually non-existent, these areas were being destroyed by population pressures and other demands at an alarming rate.

"I am convinced that 95% of the forests will be converted within 25 to 30 years", said Dr Raven, whose panel predicted that by the close of the century the only extensive areas of undisturbed forest remaining would probably be chiefly those in the western Brazilian Amazonia and in Central Africa. However even those, it says, will probably only persist "for a few more decades".

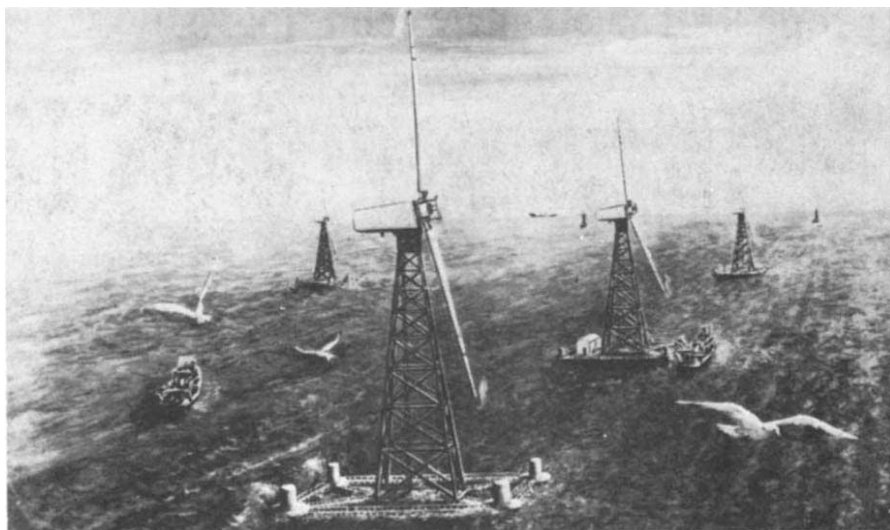
Dr Raven's conclusions were supported by Dr Norman Myers, author of a study *Conservation of Tropical Moist Forests*

commissioned by the panel, which provides a detailed account of the areas currently under threat.

According to Dr Myers, about three quarters of the tropical forests that have already been lost through the actions of millions of forest farmers over the world, who have been able to gain access to the heart of forested areas by using logging trails.

The Academy report warns that if the destruction of the forests continues at the present rate until the next century, the result will be not only widespread human misery and "alterations to the course of evolution", but also the loss of potentially useful knowledge about plant and animal species that might have been used to mitigate some of the consequences of such development.

"This knowledge is therefore not only of theoretical importance, but has critical application of benefit to human welfare, both in the tropical countries themselves and throughout the world. If tropical research is allowed to continue at its present level, it will contribute only in a minor way to the solution of the human and scientific problems to which we have alluded" the report says. **David Dickson**



Offshore windmills: can they help conserve fossil fuel?

UK energy

The prospect for ocean-going windmills

THE generation of electricity from windmills sited off the coast of Britain is technically feasible and very likely economic, according to a recent study on the assessment of offshore siting of wind turbine generators commissioned by the UK Department of Energy.

Large arrays of land-based windmills are unattractive in the UK because of the lack of large areas of unpopulated hilly land. However, the UK does have the advantage of a wide continental shelf covered by shallow waters; it also has considerable experience in offshore engineering gained from the exploitation of oil and gas from the North Sea. The study, which should be published in full later this year, has identified several possible sites where the structures for offshore windmills could be built with existing technology. A synopsis of it was presented by P.B. Simpson and D. Lindley of Taywood Engineering — the main contractor for the study — at a meeting of the British Wind Energy Association last week.

Clusters of up to 200 offshore windmills spread over an area of about 60km² could be capable of providing 600MW of electricity to the national grid, the study concludes. Their main economic value would be in the savings they would contribute to the burning of fossil fuels. Machines with 100m diameter turbines placed in areas with an annual mean wind speed of 10m/sec could generate electricity at a cost of 3.4p/kWh — compared to the current cost of about 1.5 p/kWh of generating electricity from a coal fired power station. A similar field of 80m diameter turbines and an annual mean wind speed of 9.5 m/sec could generate electricity at a cost of 5.1 p/kWh.

Unlike conventional power stations,

wind turbines have no fuel costs. The only costs to be factored into calculations of cost per kWh, therefore, are maintenance and capital costs. Moving offshore increases both considerably. According to Simpson and Lindley, the cost of the support structure for one wind turbine (excluding the turbine itself) would be £3 million. One machine would cost £34,000 a year to maintain; and support facilities onshore would have to be built at a cost of £25 million for a 200 machine cluster. Transmission costs for a 600 MWe cluster they estimated to be £171-189/kW — four times that for a conventional 600MWe power station. Construction period and operation time were assumed to be eight and 20 years respectively. They gave no total capital cost for a 200 machine cluster but Dr Peter Musgrove of Reading University, who also participated in the study, believes that a cluster with a generating capacity of up to 1000MWe could be built for roughly the same price of a conventional power station of the same capacity. All costs in the study were at mid-1979 prices.

Most of the capital cost of building wind turbines offshore would be taken up by building support structures which could withstand the rigours of the sea. So the greater the generating capacity of each machine, the more cost effective it would be. Wind turbine diameter is one of the three main factors affecting generating capacity, according to Simpson and Lindley. The other two are mean and rated wind speeds and cluster configuration. Machines spaced at anything less than fifteen turbine diameters are considerably less efficient than individual machines. In the particular sites considered in the study, mean water depth (10-25m), height of

turbine axis above the water (1-1.5×turbine diameter) and distance offshore (5-25 km) had only secondary effects.

These conclusions are based on the detailed analysis of three sites — Burnham Flats in the Wash approaches, Shell Flat off Morecambe Bay and Carmarthen Bay — chosen out of an initial 17 on technical and economic grounds. Lindsay and Simpson stress that these sites are not to be seen as automatic choices for future projects. The study did not include the question of amenity, they say, because it was felt that detailed proposals would be needed first. However, it did assume that all possible sites should be at least 5km from the coast.

If wind power is developed vigorously, thinks Dr Musgrove, it could match the development of nuclear power over the past 25 years and contribute 20% of electricity supply by the year 2000. It should be possible, he says, to build several arrays each containing 200 machines of 100m turbine diameter and capable of generating 5MW each. The net result of each array would be equivalent to one 1000 MW power station operating with a 35% load factor. Variability in wind speed would not be much of a problem because at offshore sites the wind is fairly constant over short periods. The greatest variation is seasonal but this has the advantage that the strongest wind is in winter.

To realise this potential, says Dr Musgrove, the UK should be prepared to support a government funded programme of at least £5 million per year on wind energy research for the next decade, compared to the £½ million spent last year by the Department of Energy; otherwise there is a danger that Britain will fall behind the US, Holland and Germany, its chief competitors in this field. The US and Germany are already building horizontal wind turbines of diameter up to 140m, whereas the UK has not yet received approval to build a 60m diameter horizontal axis machine even though £½ million has been spent on its design.

Dr Musgrove, however, is an advocate of vertical rather than horizontal axis machines. The problem with horizontal axis machines, he says, is that the blades are subjected to gravity loads acting in different directions and this considerably limits their size. A vertical axis machine would not have this problem and could be built to much greater diameters.

So far, however, the UK has gone no further than to design a 25m, 100-150kW vertical axis machine. A decision on further funding for that is also expected later this year. The next six months, therefore, will be decisive for the future of UK wind energy, thinks Dr Musgrove. Not only does a decision have to be taken on what should be done as a result of the offshore study, but decisions have also to be taken on the future of the 60m horizontal and 25m vertical axis designs.

Judy Redfearn

United Kingdom

Science Research Council cuts top grants

BRITAIN'S Science Research Council has been forced to reject £2.2 million worth of "alpha" quality applications for university research, a spokesman said last week.

The Science Board, one of four grant-allocating boards of the SRC (the others are Astronomy Space and Radio, Nuclear Physics and Engineering), meets three times a year to decide allocations. At its first meeting this year the board managed to stave off cuts, except for all "beta" applications — worthy research which "wouldn't disgrace the British taxpayer" but without the star quality of the alphas. Beta research — about a quarter of all applications — has not been funded for the past three years.

But at the Science Board's recent second allocation of the year, it proved impossible to avoid cuts even in the alphas. A total application for £7.1 million was cut back to £4.9 million by applying an across-the-board 30% cut to all of the Board's 10 committees. The committees range from basic biology, maths, physics, and chemistry to science-based archaeology and cognitive science.

The Board spends some £16 million annually on direct research grants to universities, compared to about £3 million each for the astronomy and nuclear boards and £19 million for the engineering board. The cuts thus represent a substantial loss of direct science spending in universities.

"We are most worried about the very few people now going in to new posts in universities" said the spokesman. "We used to be able to be a little generous and allow for the enthusiasm of these 27 to 30 year olds. But there is no more room for the benefit of the doubt. We are cut to the bone."

However, the picture shows some signs of improvement. At the next meeting, the Science Board hopes to have to cut only 15 to 20% of its "alpha" applications, as a result of money beginning to be freed from long term commitments. These are principally the spallation neutron source being built at the Rutherford Laboratory, the synchrotron radiation source at Daresbury, and Britain's one third share of the Institut Laue Langevin at Grenoble. The major part of the spending on these facilities should be over in three years, releasing some £2.3 million a year; and as grants run typically over three years, some of these savings can begin to be accounted now. □



Thaumatococcus . . . sweet discovery

Gene machining sweeter gum

TATE and Lyle, the British sugar manufacturers, are to apply recombinant DNA techniques to improve production of the protein thaumatin — a substance 2,500 times sweeter than a 10% sugar solution.

Thaumatococcus is produced by the berries of the African bush *Thaumatococcus daniellii*. It was discovered in a 1968 survey for new sweeteners conducted by the UK Ministry of Agriculture; and already Tate and Lyle have plantations of the bush in Ghana, Liberia, and Malaysia producing "hundreds of tonnes" of the berries annually.

One tonne of berries produces a kilogramme of thaumatin, which at present is sold only in Japan. Japan accepts thaumatin as a "natural product" which does not require toxicity testing; other

countries require extensive toxicity tests. An application on the basis of preliminary tests by Tate and Lyle has been with the UK Ministry of Agriculture, Fisheries and Foods since January 1978.

Dr John Higginbotham, who is in charge of the thaumatin project at Tate and Lyle's Reading laboratory, estimated future markets as a tonne annually to Japan, with five times that in Europe and ten times in the US. This 150-fold increase could come from increasing the cultivated area of *Thaumatococcus* — but the plant will only propagate in hot climates. The company could avoid "politically uncertain" crops in Third World countries if it could transfer the gene for thaumatin production from the plant to a bacterium, which could then be grown in fermenters anywhere.

Professor Ken Stacey, director of the biological laboratory of the University of Kent, has taken on the task, backed by a £90,000 grant for three years. Professor Stacey thinks three years should see them through to expression of the gene; development of a production process will take longer.

The first task is to isolate the relevant messenger RNA, he says. And ultimately there may be the problem that the bacterial cytoplasm will not encourage the formation of the eight disulphide bonds required in the folded thaumatin molecule. Thaumatococcus has a molecular weight of some 20,000, around three times that of insulin.

The size of the molecule, compared to that of sugar, is one drawback to its wide application as a sugar substitute, said Dr Higginbotham. It is less mobile on the tongue, and so takes "a second or two" to register its sweetness. Lemonade, for example, sweetened with thaumatin, would taste first bitter (from the citric acid) and then sweet (from the thaumatin). Consequently it is only likely to be used in substances like toothpaste, which stay in the mouth for a long time. □

France

Reprocessing plant close to disaster?

THE French nuclear reprocessing plant at Cap de la Hague, near Cherbourg, apparently lost all electrical power on 16 April for an hour, closing down cooling systems on high active waste tanks and ventilation over nitric-acid filled reprocessing vats. Full power was not achieved for another 11 hours.

According to a report in the *Guardian*, a major catastrophe was averted only because the plant was not yet in full production. Auxiliary power was not available because the failure was caused by a fire in a transformer room through which passed both the main and back up circuits. Pierre Tanguy, Head of Safety at the French Atomic Energy Commission (CEA) told *Nature* last week that the back up circuit was not necessarily at fault but "it

would be looked at".

According to Tanguy, there was no radioactive release and no hazard to the workforce or environment. "It was nothing serious" he said. However the French union the Confédération Française Démocratique du Travail have argued in a booklet "Le Dossier Electronucléaire" that a particular danger in reprocessing is "the possibility of cooling caused by the failure of the main and back-up electricity supply". But the accident was not predicted in any official safety studies, says the *Guardian*.

COGEMA, the government-owned company which controls reprocessing in France, have said that the plant would be open again this week.

Robert Walgate

NEWS IN BRIEF

Fusion research boost at Karlsruhe

GERMANY'S nuclear research centre at Karlsruhe, which has for a long period concentrated on fast breeder reactors, has begun to turn its attention to fusion. In its 1980-3 budget Karlsruhe has allowed a 61.5% increase in fusion spending, taking it from 22.6 million DM in 1980 to 36.5 million DM in 1983.

The centre will concentrate on the technology of complete fusion reactors rather than on basic plasma physics, says a report in *Wissenschaft, Wirtschaft, Politik*. It will also construct a superconducting coil for a torus to be built at Oak Ridge National Laboratory in the US, and make some "exploratory studies" of deuterium-tritium inertial confinement.

The greatest effort at Karlsruhe will still be on the fast breeder, but the programme will increase only by 3.5%. The total Karlsruhe budget will increase by 16% from 336 million DM in 1980 to 388 million DM in 1983.

Superconducting technology, on the other hand, will be cut by 5.4%. The main sufferer will be high current superconductivity.

May launch for two Ariane satellites

THE second test flight of the Ariane launcher to place two satellites in orbit will take place between 20-30 May from Kourou, French Guiana, the European Space Agency and the French Centre National d'Etudes Spatiales announced last week. The actual three-hour launch will occur between 1130-1430 hours GMT.

Preparations for the launch, begun on 2 April, are going ahead on schedule following the arrival of the L02 launcher at Kourou. The first, second and third stages of the launcher were erected on 4 April, 8 April and 14 April respectively. The equipment bay was placed in position on 15 April.

The plan of operations has been streamlined compared to the first flight test, L01, thereby reducing the preparation time by about a month. The satellites to be placed in orbit are Firewheel, a scientific satellite designed by the Max Planck Institute, and Oscar 9, a satellite designed by the German Branch of the Radio Amateur Satellite Corporation.

Space shuttle engine test successful

THE National Aeronautics and Space Administration's space shuttle, plagued by a succession of technical problems, received some good news last week when it was reported that the shuttle's engines had

been successfully tested at full power for six minutes. In a static firing test carried out at NASA's National Space Technology Laboratories at Bay St Louis, in Mississippi, the engine operated for a total of 10 minutes and ten seconds — including the first sustained operation at its full 109% of rated power — and met all the test objectives.

Meanwhile the agency is asking private industry to perform a study of thermal protection systems which might be developed as an alternative to the present ceramic tiles which have been giving the shuttle programme severe difficulties and have caused the delay of the first flight to late next year at earliest. The purpose is to find whether any recent technological advances in reusable surface insulation might provide an acceptable alternative. The industry study will evaluate metallic and reinforced carbon compounds.

French commission approves Plogoff reactor

THE three commissioners charged with conducting the nuclear power inquiry in Plogoff recommended last week that the government go ahead with its plans to construct twin nuclear power stations on the scenic Brittany coastline of Cap Sizun. The commissioners based their decision on the observations of 121 people who visited the government's exhibits staged in "town hall annexes" set up after the mayors of the four affected communes, Plogoff, Primelin, Goulien, and Cleden-Cap-Sizun



had refused the government access to their town halls.

The "town hall annexes" had been the subject of militant protest during the 6-week exhibit from 31 January to 14 March, and had to be surrounded by mobile police forces who engaged in battles with the daily demonstrations of up to 20,000 people that were called "la messe" (the Mass) by the protestors. Only 212 of the estimated 60,000 people in the region visited the mobile vans.

French environmental groups attacked the decision, calling the public inquiry a

"grotesque hypocrisy". M. Phillipe Marchand, deputy from Charente-Maritime and Socialist Party spokesman for energy criticised the commission for "ignoring the fact that almost the entire population is opposed to the project and that a number of scientists have denounced the insufficiency and quasi-falsification of the impact studies".

A group of 20 political organizations, trade unions and ecological groups in Finistère, seat of the regional prefecture, denounced "the police over-reaction" during the inquiry and have called for the resignation of the chief of police of Quimper and the dissolution of the prefecture's riot police as "a permanent menace against the right to demonstrate". The French Council of State will take its decision on the reactors after a delay of up to eight months.

Energy conservation cut back

ENERGY conservation spending at the UK Department of the Environment, which is responsible for building regulations, will be £31.1 million for 1980-81, according to a ministerial statement in the House of Commons. This is to be compared with an allocation of £67.6 million in 1979-80, of which £51.6 million was spent. There is no longer a specific allocation for the insulation of housing built by local authorities, said the minister, and grants for assistance with private home insulation have been reduced "to reflect the pattern of demand". According to Gerald Leach's *A low Energy Strategy for the UK*, around 50% of end-use energy is used for low grade heat. A large part of this is used to heat buildings.

British PWR application imminent

A "letter of intent" to build a Westinghouse pressurized water reactor will be issued shortly, the chairman of Britain's Central Electricity Generating Board, Mr Glyn England, said last week. The letter will release "an avalanche" of technical information from Westinghouse, said Mr England. Detailed design of the reactor would then go on within the National Nuclear Corporation, in close consultation with the CEBG and the Nuclear Installations Inspectorate (which will report on safety). A firm application for consent to build would then go to the government at the end of 1981, followed shortly after by the NII safety report. The CEBG hope that the promised public inquiry on the PWR would be held by the spring of 1982, and that if the results are favourable, construction could begin at the end of that year.



On the beach . . . Westinghouse's Angra I (left) and the Angra II site (right)

The politics of power in Brazil

WITH an electricity demand of 25,000 MW and a hydroelectric potential above 200,000 MW — the Brazilian government went ahead in 1975 with a multi-billion dollar nuclear programme. Brazil's nuclear programme is now years behind schedule and in a state of general disarray, as foreign loans are used up and open dissent appears at all levels. **Maurice Bazin** reports

THE original plan, drawn up in the first half of the 1970s, in the heyday of the fanfared Brazilian "economic miracle", called for 60 nuclear power plants, producing 75,000 MW, by the year 2000. It reflected the mood for grandiose projects of that period — the Brazilian version of the American conquest of the West — symbolized by the construction of the Transamazonian Highway. The launching of these projects coincided with a period of harsh military rule and total press censorship. No sobering criticism could be heard and only the promises of the "miracle" were allowed exposure.

Today, the Transamazonian Highway has not brought the expected development. On the contrary, it has only destroyed what was there already. As for the nuclear plan, it has been reduced to eight plants by the year 2000, and there are many calls for a stop to the programme after three plants have been brought into operation, in 1995.

Nuclear energy does not have a good reputation in Brazil. The first attempt to develop a nuclear technology in 1953 ended up in a scandal. The government had convinced three German physicists who had worked for the Nazi nuclear effort during the war, to come and resume their

work in Brazil. Three gas centrifuges for uranium enrichment were to be shipped secretly from West Germany to Brazil. But the shipment was eventually embargoed by the US High Commissioner for Germany, stopping that particular avenue of development.

Until the signing of an agreement with West Germany in 1975, Brazil had remained in the nuclear sphere of influence of the United States. During the Korean war, it delivered monazitic sands to the US Atomic Energy Commission in unlimited quantities and without compensation. Any attempts to develop a national approach on nuclear energy came into conflict with US commercial interests. In the 1960s a group of Brazilian physicists and engineers calling itself the "thorium group" was doing research in Belo Horizonte on the design of a reactor which would make use of the exceptionally abundant reserves of thorium in Brazil. Their recommendations were ignored by government and their research funds cut off after four years, as negotiations for buying a Westinghouse PWR of 626 MW were getting under way. The sale of this first nuclear plant, Angra I, was confirmed in 1972. Construction is nearing completion in Angra dos Reis on

the Atlantic coast, 130 km from Rio de Janeiro. This sale marked the high point of the influence of the United States in Brazilian energy policy.

"The present Brazilian nuclear programme is almost exclusively the result of Brazilian diplomacy and has little to do with national science and technology; those have been relegated effectively to second place, or to a marginal role" wrote Luiz Pinguelli Rosa, secretary of the Brazilian Physical Society. This sour comment reflects the government's total lack of esteem for its own scientists; their suggestion of 15 years ago to develop a thorium-fuelled reactor is now even presented in official speeches as an alternative coming from the US.

The German agreement encompasses all aspects of nuclear technology, from the prospection of fissile materials to the construction of depositories for waste. There would be a whole industry, capable of operating the complete nuclear fuel cycle. But only accessory parts are under construction so far: a fuel rods conditioning plant will be operational this year, and a heavy equipment complex, NUCLEP, for manufacturing boilers, heat exchangers and possibly turbines, is also under construction.

Two parts of the agreement raised most international concern: those for enriching uranium (breaking the American monopoly) and for construction of a fuel recycling plant, capable of extracting plutonium. In its early days, the Carter administration was anxious to stop these two plants. The diplomatic tug of war is

continuing, with the United States trying to restrict the spread of "sensitive" technologies and materials and Brazil insisting on the right to include nuclear energy in its development strategy. In the words of Brazil's chief delegate to the International Nuclear Fuel Cycle Evaluation (INFCE), Ambassador Carlos Augusto Proenca Rosa: "the use of nuclear energy in developing countries was treated by INFCE in a restrictive and limited way; there are insinuations, at times, that the major contribution that developing countries could offer in the field of nuclear energy should be to increase the prospection and extraction of their uranium resource to benefit the importing developed countries . . . We firmly believe that any effective policy of non-proliferation must be non-discriminatory and must bring about measures which are universally applicable, in order not only to guarantee the correct use of nuclear energy by countries which do not possess nuclear weapons, but also to put a stop to the arms race between countries which possess them."

The suspicions repeatedly raised about the Brazilian military government's intentions to develop a bellicose nuclear capacity rely upon past aggression towards Argentina, which led to wars in the 19th century. Today, however, both countries' harsh military regimes are much more interested in signing agreements on exchange of technology, and common ventures, than fomenting regional tensions. Numerous official visits and meetings of the heads of the nuclear programmes of each country have taken place. Each time the peaceful purpose of both countries' nuclear involvement was reaffirmed. Argentina possesses the only nuclear plant operating in Latin America — Atucha I — which functions with natural uranium. It is also negotiating with West Germany's KWU to buy a new nuclear plant similar to the ones being built in Brazil. As the Brazilian programme meets with increasing delays but the industrial capacity for nuclear components develops in both countries, the two countries have entered an era of increasing commercial cooperation, and no-one talks now of any potential enemy to use nuclear weapons against.

But even if Latin America remains free of nuclear weapons, it will not remain free of nuclear waste. To a question about where nuclear waste from the Brazilian programme would be stored, Rex Nazare, acting director of CNEN, the National Commission for Nuclear Energy, responded: "this is not yet defined. But up to six months before the first plant starts operating there is still time to define the location where to deposit waste."

Finding the sites may be difficult, because Brazilian public opinion is becoming aware of the concern that this issue has raised in developed countries. The only site proposed so far is in the mountains

behind Rio de Janeiro, a place with abundant surface water and where a large proportion of the vegetables consumed in Rio are grown. With the modest redemocratization of the military regime an amount of discussion has taken place, which has led the government to abandon the site.

The government presented its justifications for the agreement with Germany as a white paper in 1977. It claimed that nuclear power was "a necessity in view of Brazil's energy needs" and because the price of oil was increasing. It claimed that, between 1940 and 1973, the proportion of imported energy rose from 15 to 40%, and that "the hydroelectric option is approaching its natural economic limit". Paulo Nogueira Batista, director of NUCLEBRAS, the state nuclear corporation, declared that the

German view

● Many of the Brazilian nuclear opposition's calculations on hydropower are "erroneous and foolish", a spokesman for the West German federal ministry for science and technology claimed in a telephone interview last week.

There are no engineers in the opposition group, said the spokesman, and so the costs and difficulty of transporting current from distant hydro stations have been underestimated. Moreover, the Brazilian government had told Germany that by 1995 all available water resources will have been exhausted. "That's why they want to go nuclear."

The contract with Brazil allows for the supply of two reactors, Angra II and III, with options for an additional six. West German participation would decrease in the later reactors, with Brazilian participation rising to 70-90%. "But we would be happy if Brazil approves the Angra III reactor later this year." The deal was important for Germany because "if the nuclear industry wants to be economic they have to produce something", and with nuclear opposition strong in Germany deals with countries such as Brazil and Argentina were attractive. Nuclear opposition in Brazil was decreasing, the spokesman believed.

Pilot plant design for a gas nozzle enrichment plant was nearly complete, and construction has begun on site, but it will proceed at a leisurely pace. For reprocessing used fuel, Brazilian chemists and engineers are in training in Germany and working on the design of a pilot plant.

Safeguards against the diversion of nuclear materials for weapons building are included in a February 1976 agreement between the International Atomic Energy Agency and the Brazilian and West German governments. The safeguards are not "full scope", but apply only to nuclear materials and technology supplied by or derived from West Germany.

Robert Walgate

hydroelectric potential would be used up by 1990.

This technocratic argument was accompanied by reassuring declarations about the safety of nuclear energy. Technical reliability was proven from the "perfect and uninterrupted operation of close to 150 plants in 18 countries with more than 900 reactor-years of commercial service. Thus nuclear energy is the only functional alternative in view of its level of technical confidence and its competitive cost of production." The first two plants to be built with German technology (Angra II and Angra III) were promised for 1982 and 1983 respectively.

In 1974 FURNAS, the electricity supply company, had elaborated a Plan 1990 for electrical energy needs. The data presented formed the basis for the technical justifications of the agreement. It posed a growth rate of the demand for electrical energy of 11.4% per year, linked with the growth of GNP. It estimated an investment cost per plant of \$500/kW and claimed a load factor of 80% for nuclear plants, while the hydroelectric load factor in Brazil is only 50%, due to seasonal variations in rainfall.

In 1979, however, FURNAS presented a revised Plan 1992, which estimated a demand growth rate of only 7.5% a year. Considering that Plan 1990 itself had stated that "nuclear plant participation would be reduced to zero" for a growth rate below 8.7%, the revised figure becomes equivalent to proposing abandoning the whole programme. It revised the investment cost up to \$1,700/kW. The load factor of nuclear plants was brought down to 65.5% based on plants operating in the West.

During the intervening five years Brazil's "economic miracle" had come to a brutal halt: GNP grew only by 4.1% in 1977, compared to 11.8% in 1975. As censorship slackened, various sectors of the Brazilian technocracy started questioning publicly the economics of the agreement. In 1979, General Dirceu Coutinho, who headed NUCLEI, the subsidiary of NUCLEBRAS which will produce isotopes, resigned and denounced the expense of the programme.

Although criticisms of the programme had been voiced by scientists as early as 1975 at the annual meeting of SBPC, the Brazilian Society for the Progress of Science, and by the Brazilian Society for the Progress of Science, and by Brazilian scientists in exile, the defection of those locally referred to as "nucleocrats" has occurred only during the past two years. As the National Congress started functioning again, it set up a Commission of Inquiry to investigate the programme. David Simon, who had headed the Angra I project and was advisor for nuclear affairs to the president of FURNAS, resigned and collaborated with the Congressional Commission, presenting detailed testimony as a technical expert. He wrote: "apart from a reduced minority of experts — mainly to be found in the world of nucleocrats — there exists a quasi unanimity in the scientific

and technical community around a set of criticisms to be made of the nuclear agreement between Brazil and Germany . . . With the present difference in cost between nuclear and hydroelectric plants, and the availability of hydroelectric sites to the end of this century, there is no need to install nuclear plants before 1990." Indeed, Professor Jose Goldemberg, president of the Brazilian Physical Society, points out that hydroelectric reserves had been purposely underestimated by a factor of two (at 100,000 MW) by FURNAS's Plan 1990.

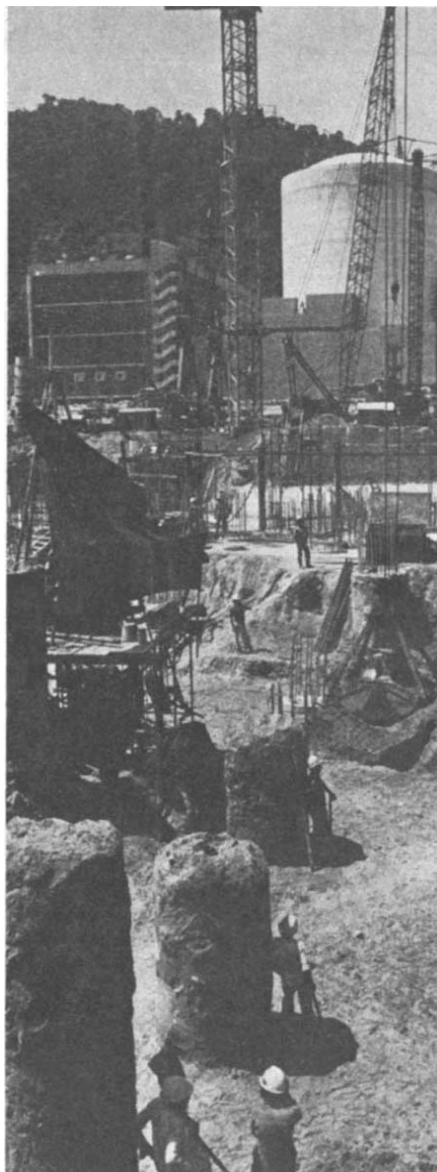
David Simon makes a detailed cost comparison between the Angra II and III projects and an equivalent 3,400 MW hydroelectric plant. While Angra I and II together will cost \$5,000 million, the hydroelectric plant would cost \$2,700 million. The fuel costs for the nuclear plants during their lifetime would be an additional \$4,000 million (water is free).

On the purely economic side, the agreement specifies that half the costs will be paid by Brazil in Deutschmarks, while hydroelectric technology would be almost entirely costed in Brazilian currency.

His testimony also pointed out that the jet-nozzle technique for uranium enrichment to be used in Brazil is still under development in West Germany (with half the costs for research and development borne by Brazil) and that its technical and economic viability have not been proved.

It is difficult to imagine the possibility of a reconciliation between the government representatives and the scientific community over the nuclear programme. Paulo Nogueira Batista, president of NUCLEBRAS, and Said Farhat, Minister of Information, called a joint press conference last month at which Nogueira discarded FURNAS' revised Plan 1992, because "the 7.5% growth rate demand envisaged in the plan is perilously below a reliable value", and Farhat emphasized "the Brazilian decision to follow through with the international agreements linked to the execution of the nuclear programme. Being a decision of the government, this is no more subject to either divergences or dissensions. To build the nuclear plants we shall spend US \$15 billion. For the nuclear fuel cycle, we estimate an investment of US \$2.5 billion."

On the other side, Professor Luiz Pinguelli Rosa, Secretary of the Brazilian Physical Society, told *Nature* that the scientific community today demands "democratic and public discussions of energy needs, involving various sectors of the population. We, as scientists, should not fall into the temptation to propose a progressive solution as a substitute for the technocrats' miracle. The illiterate majority of the Brazilian population must first obtain the means to evaluate what nuclear energy is. Before it really needs nuclear reactors, I hope that Brazil can reach the stage of a democracy with popular participation." □



Angra II columns . . . not enough

Trouble on the beach of rotting stone

ANGRA I is a standard PWR reactor of the type sold by Westinghouse all over the world under turn-key contract. Its construction made no use of the national manufacturing capacity: the share of local suppliers in the project was only 8%, made up of civil engineering works.

Start up is promised for next year. It uses enriched uranium provided exclusively by the US, paid for via a loan from the Industrial Development Corporation of South Africa. Used fuel will be shipped back to the US, so the electrical company which will operate Angra I will not

comment on wastes on fuel reprocessing.

The doubling of the construction time is blamed on the agreement with West Germany to build two plants (Angra II and III) on the same site. Besides the obvious dangers involved in having three nuclear plants on one beach, there were other problems. While the water table was being lowered to construct the foundations, the already existing buildings slid 10 millimeters from their position. It was decided to build an "armoured diaphragm wall" around the site, 250 meters long and 17 meters deep. As work proceeded on the anchoring of foundation columns for the reactor platform, it was discovered that the ground below the beach was scattered with huge boulders which had to be pierced to reach bed-rock at a depth of 60 meters. This difficulty could have been anticipated had the engineers recalled the difficulties encountered while building the road to Angra, when a tunnel collapsed to form a huge open trench, through which the road now passes; or had they recalled that the beach's name, "Itaorna", comes from Tupi, the language most spoken in Brazil 250 years ago, meaning "rotting stone".

Setbacks also affected the construction of Angra II. Last year, the National Commission on Nuclear Energy, CNEN, which is responsible for safety, decided that the 280 columns already in the ground were not strong enough against earthquakes. As a result, the construction of the reactor base plate was halted for a year, and CNEN ordered an additional 88 columns and reinforcement of 202 of the already existing ones.

It has been pointed out that CNEN's director has always been considered pro-American, while NUCLEBRAS' is pro-German; stronger rifts could exist between them than soil vibrations.

The overall financial charges from these modifications and delays amount to \$320 million, mainly in the form of interest payments. All these costs will be borne by the electrical company FURNAS alone; it has already announced that it will raise its prices by 60% when Angra I comes on-line next year. There are clauses in the German contracts which pass the cost of technical difficulties onto the Brazilian user company; these clauses remain secret and a Sao Paulo newspaper was seized last year for publishing one of them.

The financing of the German programme is the largest loan ever obtained for a Brazilian project: it amounts to \$1,700 million, and with costs tripled from the original predictions, money is getting short. No electrical company is willing to take on the responsibility for the construction of any further plant beyond Angra III. The government and NUCLEBRAS are pressuring CESP, the utilities company of Sao Paulo, to build a fourth plant (the third of the German agreement), but CESP prefers to build more hydroelectric plants at a quarter of the cost of nuclear plants. □

CORRESPONDENCE

Swiss vote did not decide disposal issue

SIR,—In the issue of 27 March, page 294, under the heading "Power dissenters through Europe", was a report on an anti-test drilling vote in the Swiss village of Högendorf. The assertion that this vote "may have blocked effectively further nuclear development in Switzerland" is unfounded and should be corrected.

The cooperative Nagra has been charged by the Swiss government and the utilities with nuclear power plants, with working out a national disposal concept by 1985, and test drillings are an integral part of the project plans. Högendorf is one of 12 sites in north-east Switzerland for which drilling applications will soon be submitted.

Because nuclear waste disposal is recognized to be a national issue, it was clearly specified in the revised nuclear law accepted by the Swiss public in 1979 that the government is directly responsible for decisions on disposal questions. This responsibility includes any preparatory investigatory work such as the proposed test drillings.

The vote in Högendorf was of a consultative nature. This implies that the government will take such indications of local opinion into account as far as possible, but must also consider the broader, national issues involved. While the applications are being considered project work on waste disposal is continuing on a broad front including planning of further drilling in geological formations other than the deep crystalline rocks currently being investigated.

Yours faithfully,

H. ISSLER
C. McCOMBIE

Nationale Genossenschaft für die Lagerung
Radioaktiver Abfälle, Baden, Switzerland

UK science budget: now the bad news

SIR,—Your report (3 April, page 391) that the science budget will be relatively stable for the next four years misses one important point. Less than half the funds of the Agricultural Research Council comes from the science budget; the majority comes from the Ministry of Agriculture, Fisheries and Food. This has been cut for 1980/1 by an average of 6% but some research institutes will, at constant value, have lost 15% of their 1979/80 budget level by 1981/2. There is no promise of stability for this funding and our fear is that it will decrease further.

I am not out, at least on this occasion, to indulge in special pleading on behalf of agricultural research, or for research in general. It is important, however, that you and your readers are aware that for at least one of the research councils the outlook is neither as good nor as stable as you implied.

Yours faithfully,

JOHN GOODIER

Agricultural Research Council, London

Genothetically speaking

SIR,—The use of new techniques in molecular biology has allowed the fragmentation of the genome of an organism and the introduction of its pieces into adequate hosts where the entire genome can be stored for the desired use when required. This collection of genes is termed in the laboratory jargon and also in the specialized literature "a library of genes". Library is defined in several English dictionaries both British and American as "a place set apart to contain books for reading,

study or reference; a collection of books, especially one for use by the public." Therefore it seems that the expression "library of genes" is inadequate. (This fact becomes more obvious taking into account that library is specifically derived from the Latin word for book, *liber*.)

The discovery of new phenomena or the introduction of new concepts may need the introduction of new words to characterize them precisely. I feel that in the case mentioned above the creation of a new word is fully justified. I would like to propose the word *genothèque* to designate the collection of genes of an organism introduced into an appropriate host species. This short word is derived from *gene* and the greek root *θηκη* (box). This last root appears with the same meaning as that proposed in several English scientific words (perithecium, spermatheca) and in other languages in the word for library (biblioteca, bibliothèque, Bibliothek.)

If we have disotheque, why not genothèque?

Yours faithfully,

CARLOS GANCEDO

Instituto de Enzimología del CSIC
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Autónoma, Madrid-34, Spain

Ke Xue not pucka

SIR,—In his letter (3 April, page 394) Gerard Piel disputed with my statement in a recent *Nature* article that "There are no cover-to-cover translations into Chinese" of foreign scientific journals as yet, by pointing to *Ke Xue* which is the Chinese edition of *Scientific American* being circulated in China. It needs however, to be clarified that this magazine is not regarded by the Chinese as belonging to the category of primary journals, which was the object under examination in my article. Another reason why I did not mention *Ke Xue* was that its publication is a joint exercise and not a solely Chinese effort.

Yours faithfully,

T. B. TANG

Darwin College, Cambridge

Wallis's biblical gesture

SIR,—In Kings Norton's obituary (20 March, page 288) of Sir Barnes Wallis, he mentions that Wallis presented his £10,000 award for his wartime dambusting bomb to a charitable foundation (to help educate the sons and daughters of men who died serving with the RAF). Why he did this deserves to be known. In the book *The Dam Busters*, Paul Brickhill relates the tremendous effort expended for the German dams' raid. When it was radioed by 617 squadron that the Mohne and Eder dams were breached, Sir Wallis was ecstatic that the huge work succeeded. Afterwards, though, he learned that 50% of the attacking planes were lost with the death of 53 very young men. He was shocked and saddened, and said "Oh, if I'd only known, I'd never have started this!" Many years later he was presented the above award. When Paul Brickhill asked him why he gave it away, he replied: "My dear chap, go and read your Bible, turn up Samuel II, Chapter 23. You probably haven't a Bible, so I'll tell you this story about David.

"He was hiding in the cave of Adullam after the Philistines had seized Bethlehem, and in his anguish he said, 'O that one would give me a drink of water of the well of Bethlehem, which is by the gate!' Now the three mighty men who were his lieutenants were with him, and I'm dashed if they didn't fight their way through the Philistine lines and draw a goatskin of water out of the well by the gate.



They fought their way back and took the water to David in the cave, but when they told him how they had got it, he would not drink it. They asked him why, and he said: 'Is not this the blood of the men that went in jeopardy of their lives?'"

Yours faithfully,

P. D. MORLEY

University of Texas, Austin, US

Optical effects in taxis

SIR,—I wonder if any of your readers can explain a strange optical perception effect I've noticed when travelling in a London taxi-cab? No-one I've described it to has a simple explanation; and I assure you it is not restricted to trips taken after alcoholic lunches. The effect is most visible at night.

London taxis, like many others, have a vertical glass partition between the passenger seats, which face forwards, and the driver. Behind the passenger is the rear window of the cab, through which can be seen the receding road, street lights, other vehicles and buildings.

The glass partition acts like a partial mirror; in it one sees a reflection of the view through the rear window, and through it the back of the driver, the meter, and part of the view through the front cab window.

The effect — and when one has spotted it it is quite marked — is that the objects reflected in the partition glass (ie those in the view through the rear window) appear to be receding very fast, much faster than the cab is apparently travelling judging by the view forward through the partition. Subjectively, the impression is of about twice the cab's real speed. I first noticed it with the reflection of traffic lights receding at night as the cab passed them.

The effect disappears completely if one turns around and looks directly out of the rear window; there one's perception is roughly as expected. Turning back to the reflection of the same view, the recession seems to speed up again.

The only explanation I can think of is that the fixed angular size of most of the through-screen images (the back of the driver, the cab) and the increasing angular size of others (the road forward) provides a reference frame against which the decreasing angular size of the reflection is correctly perceived. Turning around, the frame of the window is too peripheral to provide an effective reference, and the brain's mechanisms for maintaining a constant perception of size of an object despite its distance come into play. Is this a possibility?

Yours faithfully,

ROBERT WALGATE

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NEWS AND VIEWS

Pores for thought

from Peter Newmark

MEMBRANE pores, like teeth, can be probed, filled or extracted. Each approach can be useful in the investigation of pore structure and function and each has its advocates. In time the evidence obtained by different approaches should merge but to judge by a recent meeting*, this has not happened yet.

The sodium and the potassium channels that generate action potentials in nerves continue to provoke a good deal of interest and controversy more than a quarter of a century after Hodgkin and Huxley put their electrophysiology on a mathematical basis. The Hodgkin-Huxley model proposed that each channel had a gate which was normally closed but could be opened by depolarisation of the nerve membrane. They postulated that the gates contained three charged particules, and that their successive movement in response to depolarisation allowed ions to start flowing through the channels. It was predicted that their movement should generate a detectable gating current preceding — and much smaller than — the ionic current. The detection of gating currents became a reality in 1972 when both C. Armstrong and R. Keynes recorded them from the sodium channel of the squid giant axon. Keynes (University of Cambridge) now believes that his original conclusion that the gating currents associated with the activation of sodium channels fitted the Hodgkin-Huxley three particle model is wrong. He now gives a mathematical description of the time course of the changes in gating current in terms of two exponential functions and a single constant. This equation is not compatible with the three particle model but nor does it lend itself to any other obvious physical model. Armstrong (University of Pennsylvania) meanwhile obtains what appear to be somewhat different gating currents from the same preparation and describes them with a far more complex equation than that of Keynes. But, like Keynes, Armstrong concludes that the simple Hodgkin-Huxley model is an inadequate physical explanation of gating currents. His tentative model is considerably more complex and also

without an obvious physical counterpart.

The problem of finding physical counterparts to electrophysiological data is that of how to relate an alteration in electric field to a movement of the gating particles — in biochemical terms a conformational change — which brings about the opening of an ion channel. Also to be explained is how, for example, a low pH acting from the inside, and only from the inside, of frog muscle almost completely prevents the closing of sodium channels without having any effect on their opening and the peak sodium flow through them. (W. Nonner, University of Washington, Seattle — see *Nature* **284**, 360; 1980).

Biochemists, at least, believe that all will be revealed once they have purified the protein that encompasses the channel in question. For that reason Y. Ovchinnikov (Shemyakin Institute, Moscow) set a considerable workforce to the task of purifying the sodium channel protein from the nerve in the muscle of the walking legs of 35,000 crabs. The protein thereby isolated has an overall molecular weight of about 250,000 and consists of four subunits of which a 76,000 molecular weight component associated with a 56,000 molecular weight component is the target for a photoaffinity labelled scorpion toxin. However, W. Catterall (University of Washington, Seattle) has a different photoaffinity-labelled scorpion toxin which picks out a much larger protein (250,000 molecular weight) from either neuroblastoma cells or rat brain synaptosomes together with a 32,000 molecular weight protein from the latter. These proteins are shown to be associated with the sodium channel both because mutant neuroblastoma cells that lack sodium channels also lack the 250,000 molecular weight protein and because either depolarisation or unlabelled toxin are capable of blocking the photoaffinity labelling of synaptosomes. (*Proc. natn. Acad. Sci. U.S.A.* **77**, 639; 1979).

The biochemistry of calcium ATPase is far more advanced. Of its 1000 amino acid residues 830 are already known (N. M. Green, National Institute for Medical Research, Mill Hill) and from that and other evidence it is possible to infer how the protein is threaded in and out of the sarcoplasmic reticulum membrane in which it sits. Perhaps with the complete

sequence it will be possible to see whether the calcium channel is formed between dimers or through a monomer. Intuitively, Green favours the monomer theory as does J. Kyte (University of California, San Diego) who argued cogently for a reappraisal of the current dimer theory of channel formation for sodium, potassium ATPase.

The acetylcholine receptor is another protein of interest to both biochemists and electrophysiologists. After two molecules of acetylcholine have bound to their receptor, a water-filled pore opens allowing ions to pass through. Purification of the protein yields a major 40,000 molecular weight protein and several minor components whose roles seem to be becoming less clear. In particular J.P. Changeux (Pasteur Institute) reported that the minor component which he had previously thought to be the 'ionophore' could not be involved in ion flow because it could be removed by alkaline extraction without loss of activity.

The function of two other minor subunits is also unclear and attention is now firmly focused on the 40,000 molecular weight major subunit. Its amino acid sequence, derived either from the purified protein or from DNA, reverse transcribed from messenger RNA, should be available shortly.

Meanwhile the goal of consistently and convincingly reconstituting purified acetylcholine receptor in lipid bilayers whilst retaining ion flux and binding parameters continues to elude P. Mueller (Eastern Pennsylvania Psychiatric Institute) and many others. Changeux, however, claimed some success in reconstituting into small lipid vesicles the acetylcholine receptor of alkaline-treated, solubilized synaptic membranes of the Torpedo electric organ. The key to success, he claimed, was the addition of exogenous lipid.

One good reason for wanting purified reconstituted acetylcholine receptors is that it will enable more sophisticated electrophysiology to be carried out. But even with *in situ* receptors, great advances have been made with the advent of single channel recording. This technique, pioneered by E. Neher and B. Sakmann of the Max-Planck-Institute for Biophysical

*An International Titisee Conference on Membrane Channels held at Titisee, March 6-9 1980, sponsored by Firma Dr. Karl Thomae GmbH.

Chemistry, Goettingen has recently been applied in that Institute to desensitized channels of extra-synaptic membranes of denervated frog muscle. The desensitized form of the channels is an inactive state adopted about 10 sec after the channel has converted from closed to open form in the presence of high concentrations of acetylcholine. Sakmann reported that desensitized channels can suddenly become active again, rapidly fluctuating between open and closed states for less than a second before returning to inactivity. After a very brief delay the cycle may be repeated; after several repeats, there is a prolonged period of inactivity before bursts of activity reappear.

Neher reported the considerable technical achievement of extending single channel recording to potassium channels of the squid axon, although not under normal physiological conditions. This is the first time that the opening and closing of a single channel in a biological membrane, other than those induced by acetylcholine, has been recorded. Currents of 1.7 to 7.0 pA and a mean opening time of 12 msec were recorded. More interesting was the fact that short interruptions could be seen during the open time recording, indicating a temporarily closed condition between the closed and open states. On average there were 1.8 interruptions per open channel recording. According to Neher this refutes the Hodgkin and Huxley model which, for potassium channels,

would predict 0.4 interruptions per channel.

Single channel recording is becoming an extremely important technique for confirming and extending what has been learnt from gating currents and fluctuation analysis in the last few years. It is technically demanding particularly in terms of obtaining sufficiently good seals between the recording pipette and the tissue membrane. But there is every reason to believe that the first recordings of single sodium channels will be reported soon and will be followed by those of channels induced by a variety of neurotransmitters.

An utterly different approach to determining single channel currents, but one applicable only to the much studied model of gramicidin A channels induced in lipid bilayers, was reported by D. Urry (University of Alabama Medical Centre). By means of sodium-23 nuclear magnetic resonance he was able to demonstrate two sodium binding sites and to derive the relevant rate constants. From them and using a model based in part upon the X-ray diffraction data of Koppe *et al.* (*Nature* 279, 723; 1979) Urry then calculated a single channel current which agreed well with observed values. The validity of this approach having been proven to Urry's satisfaction, he expects soon to determine the actual positions of the binding sites within the channel. That will perhaps conclude the exhaustive studies, exhaustingly reported, of the exact way in which sodium ions make their way through the gramicidin channel. □

Northern Hemisphere continents increased there may have been a sudden input of phosphorus to the oceans due to leaching from the then dry continental shelves. The increased phosphorus levels would stimulate increased photosynthetic activity in the oceanic biomass and thus lead to lowered levels of CO₂ in the atmosphere. This mechanism provides a link between Northern Hemisphere glaciations and Southern Hemisphere temperatures and also indicates the importance of internal responses within the biosphere and hydrosphere for the enhancement of climatic anomalies. It does not, however, explain the origin of the decreased temperatures that triggered the original continental glaciations.

The extreme importance for climatic change of the oceans and particularly the biomass within them was re-emphasized by J. Woods (Southampton) and B. Bolin (Stockholm). Woods presented new and exciting data which indicate that there is a large-scale, but previously neglected, interchange of both water and heat between the major world oceans. Bolin's work on the carbon dioxide budget also underlines the desperate need for a fuller investigation of the oceanic contribution to global sources and sinks of CO₂. Bolin and J. Smagorinski (Princeton) both noted a 4-year fluctuation in the observed levels of atmospheric CO₂ which becomes apparent when the annual oscillation forced by industrial output and biospheric uptake is removed. There is a strong possibility that the atmospheric CO₂ levels are responding to increased uptake by the oceanic biomass during periods of enhanced upwelling. This is another link between atmosphere, climate, sea surface temperatures and the biosphere. One is tempted to reconsider these complex interrelationships in the light of Lovelock's 'Gaia Hypothesis' (Oxford University Press, 1979).

In view of the extremely complex nature of the response of the biosphere to climatic changes it was reassuring to hear from H. Fritts (Arizona) that, given a large enough sample size, biological data (especially from tree rings) could be shown to provide satisfactory reconstructions of past climates. The elegance and strength of the mathematical techniques used in this type of reconstruction (which now allows Fritts to draw surface pressure charts for earlier epochs) may lead to biological data superseding the traditional historical data sources for climatic reconstructions. Although the response of trees to climatic variation may be complex, the signal is, at least, not further confused by intentional modification of the true situation (as is known to have occurred in certain mediaeval manorial reports of crop yields written for absent landlords). Climatic reconstruction and monitoring thus cannot be successfully developed without detailed consideration of both anthropogenic and environmental systems with which the atmosphere is intimately linked. □

Peter Newmark is acting editor of *Nature*.

Climatic perturbations

from A. Henderson-Sellers

REINFORCEMENT of climatic perturbations by the natural feedback mechanisms in the atmosphere was a predominant theme of the First International School of Climatology*.

Probably the most exciting indications of internal feedback response amplifying natural climatic variations were the results presented by J. Namias (Scripps Institution) and W. Broecker (Lamont Doherty Observatory). Namias' theories are the culmination of many years of scientific investigations of synoptic meteorology. He presented examples from the USA of atmospheric reinforcement of anomalous situations in various conditions from arid to arctic. (There is an interesting analogy here with the Northern European Drought of 1975/76 which, it has since been suggested, may have persisted because of the extreme conditions of low soil moisture and high reflectivity which it had itself produced). Perhaps more important for those interested in the onset

of ice ages was Namias' further observation that baroclinic development in arctic regions seems to lead to the 'steering' of depression systems along the snow/ice boundary. This gives rise to a positive feedback mechanism in which any precipitation which occurs will 'feed' the snow advance.

The second novel case of an ecosystem response tending to enhance climatic perturbations was described by Broecker. Recent results suggest that the level of carbon dioxide in the Earth's atmosphere was considerably lower (~200 p.p.m.) during glacial epochs compared with the present day (320 p.p.m.). This decreased level of CO₂ would, of course, tend to lower average surface temperatures by decreasing the 'greenhouse effect'. However, the lowered CO₂ level may itself be a result of the onset of continental glaciations. Broecker believes that when sea levels fell as the ice mass on the

*A NATO Advanced Study Institute, held at Erice, Sicily on 9-21 March, 1980. The proceedings will be published by Reidel.

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ON the premise that the climate has changed in the past and thus may well change in the future a Government 'think tank' — the Interdepartmental Group on Climatology — was set up to 'consider whether the United Kingdom was putting sufficient effort into studying the climate'. This group, under the chairmanship of Sir Kenneth Berrill has just delivered its first report*.

The review of the present state of knowledge on climatic change (with special reference to the United Kingdom) is clear, succinct and realistic, but restricted to short-term changes. Major emphasis is given to the carbon dioxide issue (fossil fuel burning and deforestation leading to increased atmospheric carbon dioxide leading to possible global warming), and interesting estimates are given of the economic effects of a 1°C warming (a saving of £250 million per year through reduced energy demand), and the cost of a single severe winter (in excess of £80 million). This section ends with the rather optimistic statement that "within a few years sufficient progress will be made" in predicting the effects of global climatic change on the United Kingdom that climate trends will be able to be considered in future Government planning. This statement implies an understanding of the causes of natural and anthropogenic climatic change which we do not have at present, and would be viewed with scepticism in some circles.

As a review of the present state of knowledge, the Report is deficient in its neglect of studies of climatic change which are based on historical or proxy

Climatic change

from T.M.L. Wigley

data. This is disappointing since a number of world authorities in these areas are British, and British scientists have been involved in some important recent works. For example, the verification that orbital changes have a major controlling role in determining the cycle of ice ages (Hayes *et al.*, *Science* 194, 1121; 1976) is believed by many to be the major work on climatic change this decade. It might be argued that long-term climatic change (the topic of the Hays *et al.*, paper) was not of any immediate relevance. However, analysis of the past few hundred thousand years has revealed evidence that very rapid climatic changes have occurred in the past (see, for example, Hollin *Nature* 283, 629, 1980). Such changes, if they occurred today, would undoubtedly have a dramatic impact on society. The Report's emphasis on the instrumental record and computer modelling tends to reflect the work on climatic change which is being conducted in the U.K. Meteorological Office. As such it creates an unbalanced impression of work in progress in the U.K. as a whole. This impression is reinforced by the summary of research funding. Only £1 million per year is expended directly in support of research on climatic change: divided into studies of past climates (£¼ million per year) and climate modelling (£¾ million per year). From other figures quoted in the Report, it is apparent that the bulk of this expenditure is within the Meteorological Office. But how good are these estimates and how well do they reflect the work on climatic change which

is being carried out in the United Kingdom?

If one were to consider all research into past climates which is being conducted in the United Kingdom, then the figure of £¼ million per year would be an underestimate. However, a significant amount of funding in this area comes from outside the U.K., and it seems that the Report has considered only U.K.-funded research. Since this restriction has not been stated, the figures tend to be misleading — and a more complete analysis would be of great interest. But how can one properly review all U.K. research into the many facets of climatic change? Funding figures can be deceptive, even if correctly assessed, since certain items of research (such as climate modelling) are extremely expensive. How many scientists are working in the area, how much material is published, and how are these figures distributed amongst the various government bodies, the universities and private industry? These are important questions which the Group has not yet considered.

It is a pity that this Report is restricted to a review of research within, or funded by, the Government. By so doing, a considerable amount of research which is important in the study of climatic change has been neglected. Unfortunately, except in the title no statement is made of the restricted nature of the Report, and, because of this, it can only be misleading to the uninitiated. The Interdepartmental Group on Climatology has, however, not been disbanded, and we can, perhaps, look forward to a more comprehensive review at a later stage. □

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*Climatic change. Its potential effects on the United Kingdom and the implications for research, London, Her Majesty's Stationery Office, 1980.

Sign language and spoken language

from James E. Cutting

WHAT is the nature of language? This question has taken on new vitality with our increasing recognition of a whole family of languages previously ignored by linguists, philosophers, psychologists, and neurologists. These languages are the sign languages of the world used by communities of the deaf in enclaves scattered about the globe. These languages are, we suspect, as varied and different as are spoken languages; they are, we now recognize, not at all parasitic on the spoken languages around them; and they have been, regrettably, little studied so far.

How do sign languages differ from spoken languages? How are they similar? What is characteristic of language in general, encompassing both signed and spoken languages? These were among some of the fundamental questions which were discussed at a recent conference*.

By comparison with spoken languages, little is known about sign languages and the only substantial study to date is that of Klima and Bellugi (*The signs of language* Harvard Univ. Press, 1979) on American sign language (ASL). Cross-linguistic study with Chinese Sign Language, French Sign Language (FSL), British Sign Language (BSL), and others has only just begun. One of the interesting facts about these languages is that users of ASL find FSL much more intelligible than BSL, undoubtedly because the American and French sign languages are historically related to one another and quite independent of BSL. This is important

evidence for the independence of the signed from the spoken languages that surround them.

At first glance one might suspect that sign and speech are quite different, if for no other reason than we have two hands for articulation but only one tongue. Interestingly, however, in sign language bilateral gestures in which either opposition or symmetry between movements of the two arms is used seem to be rare. Historical analysis suggests that the evolution of signs may be in the direction of simple one-handed forms.

We can consider sign to entail the shape and movement of the dominant hand (signers can be right- or left-handed) from one place of articulation to another with coordinate gestures of the head and shoulders — just as speech may be thought of as movement of the tongue from one place of articulation to another with co-ordinate closures and releases of the lips and larynx. The places of articulation for signs are centered in two distinct areas. One is

*A Dahlem workshop on Sign Language and Spoken Language, held in Berlin 24-28 March 1980, under the auspices of Senat der Stadt Berlin and Stifterverband für die Deutsche Wissenschaft. The proceedings will be published later this year by Dahlem Konferenzen, edited by U. Bellugi and M. Studdert-Kennedy.



100 years ago

Seeing by Electricity

It may be well to intimate that complete means for seeing by telegraphy have been known for some time by scientific men. The following plan was suggested to us some three years ago by a picture in *Punch*. Our transmitter at A consisted of a large surface made up of very small separate squares of selenium. One end of each piece was connected by an insulated wire with the distant place, B, and the other end of each piece connected with the ground. The object whose image was to be sent by telegraph was illuminated very strongly, and, by means of a lens, a very large image thrown on the surface of the transmitter. Now it is well known that if each little piece

of selenium forms part of a circuit in which there is a constant electromotive force the current passing through each piece will depend on its illumination. Hence the strength of electric current in each telegraph line would depend on the illumination of its extremity. Our receiver at the distant place, B, was, in our original plan, a collection of magnetic needles, the position of each of which was controlled by the electric current passing through the particular telegraph wire with which it was connected. Each magnet, by its movement, closed or opened an aperture through which light passed to illuminate the back of a small square of frosted glass. There were, of course, as many of these illuminated squares at B as of selenium squares at A, and it is quite evident that since the illumination of each square depends on the strength of the current in its circuit, and this current depends on the illumination of the selenium at the other end of the wire, the image of a distant object would in this way be transmitted as a mosaic by electricity.

From *Nature* 21 April 22, 589; 1880

around the face, generally centred between the eyebrows and mouth and off-center to the cheek ipsilateral with the dominant hand and the other is in front of the signer with the nondominant hand often used as an articulator surface for the dominant hand to play against. The coordinate gestures of the head and shoulders, however, play a different part in sign from the part played by lips and larynx in speech. Head and shoulder movement seems to serve, in part, grammatical purposes, such as denoting relative clauses, and they play no role at a sublexical level. The lips, on the other hand, play no grammatical role in any known spoken language, but are intimately involved in sublexical units.

Lexical items in speech are composed of phonemes. English has about 50. Lexical items in sign are composed of formational parameters — handshape, place of articulation, movement, and orientation — that yield somewhat more than 50 cheremes (Stokoe et al, *A dictionary of American Sign Language on linguistic principles*, Gallaudet College Press, Washington, 1965). These parameters have a somewhat different status in sign than phonemes, or even syllables, in speech; yet slips of the hand are often parameter-based just as slips of the tongue are often based on phonemic and syllabic structure. Virtually all hand configurations can go with any movement, with virtually any place of articulation, yielding as many as 10,000 possible ASL signs.

The interesting point is that these parameters are arrayed simultaneously, whereas the phonemes and syllables of speech are strewn out in time. It may be that the difference in modality has encouraged differences in manner of composition of linguistic units. Visual display may allow a more simultaneous

arrangement, and auditory display a more sequential one. Sign languages, particularly ASL, appear to use this simultaneous, spatial character in many ways.

For example, morphological functions can be overlaid on lexical items without necessarily increasing the amount of time taken to produce the sign. This is done by modulating the movement parameter of the sign such that both the original movement and the modulation can be seen. Consider the ASL sign for BLACK, articulated in citation form with the forefinger of the dominant hand extended as the hand sweeps across the eyebrows from the contralateral to ipsilateral side of the face. To change this to VERY BLACK the sweep is made with an accelerated beginning; to change it to BECOME BLACK the sweep is made with an initial tenseness or hesitation; to change it to HABITUALLY BLACK it is made repeatedly; and so forth. In this manner the lexicon of the language is syntactically enriched through the use of morphological additions quite unlike those used in English. Moreover, these operations are performed on the base sign in an essentially simultaneous layering. Spoken languages seem to be rather impoverished in terms of this kind of layering; the best candidates seem to be the vowel harmonies of languages like Finnish, the insertion of different vowels into consonant groups according to grammatical function in Hebrew, and the use of tone for grammatical purpose in certain dialects of Chinese.

A second example of the use of space by sign that has simultaneous character may be found in anaphora and pronominal reference. In sign discourse, the signer "lays out" in space for the sign recipient the actors and objects that are referred to. This is done laterally, left to right or vice versa, indicating a single place for each person or object to occupy, using as many

as four, five, or more locations. These locations are set up as they occur in discourse, and both signer and sign recipient remember their places. In this manner, pronouns analogous to *she* or *it* are actually locations in front of the signer, set up arbitrarily in discourse and held there simultaneously as long as is convenient, and removed when necessary. Spoken languages seem to have no analogue to this, partly because acoustic space may not allow tone heights to be used pronominally. It would simply be clumsy and confusing.

Thus, simultaneous layering of linguistic units and simultaneous display of anaphoric relations may be ways in which sign languages have been encouraged by modality to structure themselves. Yet simultaneity of display in vision may have costs in sequentiality. Consider proposition rates. Semanticists have used the notion of a proposition as a unit of discourse. For example, *John is a secretary* is a sentence with one proposition, and *John is a tall secretary* is a sentence with two, roughly corresponding to *John is a secretary* and *The secretary is tall*. There are many schemes for counting propositions, and semanticists have long debated which way is the best, but regardless of the scheme, sign languages (at least ASL) seem to convey only about two thirds of the propositions per unit time that spoken languages do. Thus, it may be that the visual modality encourages simultaneous layering precisely because it is constrained sequentially.

Sign languages force a broadening of our notions of what language is. Indeed, we are often surprised by what we find in sign. Surely more surprises are in store. For example, we currently have little idea of what difference there may be between apraxia and aphasia of the deaf who have suffered cerebral impairment. Yet these are useful diagnostic categories for the hearing in clinical settings. In addition, we have no real idea why second-generation signers are better than first-generation signers — that is, their language is richer — when in fact the second-generation users learned their language, by and large, from first-generation users. Answers to these and other questions will come. What will also come with increasing inquiry into sign, and this is surely the best byproduct of all, is an increasing recognition and liberation of the deaf. Traditionally, deaf peoples have had to learn in oralist mode, one particularly ill-suited to their handicap. At best they have been tutored using a language form still far removed from native sign. In the United States this form is called Signed English, which takes the lexical items of ASL and strings them together according to English syntax. Even in this situation deaf signers have been overtly discouraged from using their native language. Increasing inquiry into the sign languages of the world by deaf and hearing researchers can only help the deaf to realize their full potential. □

How long is the charmed lifetime?

from Graham Shaw

BY remarkable techniques akin to the systematic location of needles in haystacks, experimental physicists have made considerable progress towards determining the lifetimes of charmed particles. In so doing, they have confirmed yet another fundamental feature of the unified theory of weak and electromagnetic interactions (see *Nature*, 282, 131; 1979).

According to theory, the lightest charmed particles should decay by weak interactions, since charm itself is conserved by the more powerful strong and electromagnetic interactions. And these weak interactions should be the same as those responsible for familiar processes like muon decay, or nuclear beta decay, with precisely the same coupling constant. However, because the charmed quarks are so much heavier than the strange quarks to which they decay, the density of allowed final states available (the 'phase space') is much greater in charm decays than in, for example, muon decays, so that the predicted lifetimes are much shorter. How much shorter depends on the charmed quark mass, but with reasonable values, lifetimes τ in the range 10^{-12} to 10^{-14} s are obtained, compared to 2.10^{-6} s for the muon lifetime.

Turning to experiment, such short lifetimes $\tau \sim 10^{-13}$ s present a serious challenge, since in a typical charm production experiment they imply decay lengths of a few hundred microns. This distance is of the same order as the bubble size in a typical bubble chamber, so that the resolu-

tion required is on the edge of what may be achieved with this technique. Very much better spatial resolution can be obtained by observing charmed particle production and decay in photographic emulsion stacks. In this case, the problem is not to resolve the details of a given event, but to find it. The events are relatively rare, and in general occur deep in the stack, so that it would take innumerable man years of effort to locate them in sufficient numbers to be interesting. A method is required to locate the needles in the haystack, without sifting every straw. This is provided in a series of so-called hybrid experiments by placing an array of electronic counters and/or a bubble chamber behind the stack (see *Nature* 279, 287; 1979). These can be used to detect the particles produced together with the charmed particle, and the decay products of the said particle, when they emerge from the stack. They serve a triple purpose: to select out possible charmed particle events from more numerous backgrounds; to identify and measure the momenta of the emerging particles; and by tracing back the flight paths of the particle into the stack, to locate the small region in which the event occurred.

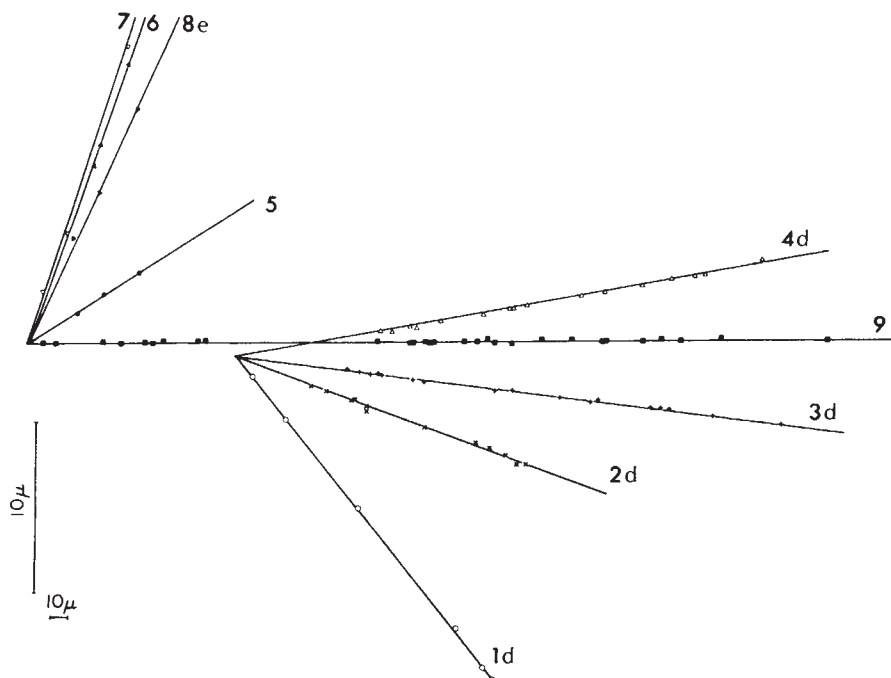
An example of an event located in this way is shown in the figure, with best fit lines drawn through the measured positions of grains in the emulsion (Adamovich *et al.*, *Phys. Letters* 89B, 427; 1980). In this experiment, an 80 GeV tagged photon beam is incident from the left, and the event clearly indicates a neutral particle being produced at the first vertex, and decaying at the second. Information from the external counters indicates $\pi^+\pi^-\pi^-K^+$ as

the most likely identification for the decay products 1d, 2d, 3d, 4d, and leads to a mass for the decaying particle of 1866 ± 8 MeV/c², coincident with the mass of a charmed $\bar{D}^0$ meson. On the basis of this and other detailed considerations, the authors conclude that the production and decay of a $\bar{D}^0$ meson has almost certainly been observed. The distance between the production and decay vertices is measured in the emulsion as $122.7 \pm 2.2 \mu\text{m}$, with a corresponding decay time of $(2.26 \pm 0.05) 10^{-14}$ s.

Similar events have also been seen in several other hybrid experiments (see Voydovic, *Proceedings of the International Symposium on Lepton and Photon Interactions at High Energies*, Fermilab, Batavia, in press). In particular, a group working at Fermilab with a neutrino beam, has reported as many as ten events in which the decaying charmed particle can be identified (Prentice, *ibid.*). These include two Λ_c baryon decays with a mean decay time of about 4.10^{-13} s, four D^+ or F^+ meson decays with a mean decay time just under 10^{-12} s, and four D^0 meson with an appreciably shorter mean decay time of about 7.10^{-14} s.

These experiments, and others like them (Voydovic, *ibid.*) clearly show that charmed lifetimes lie in the expected range, providing further striking evidence for the universal coupling of the weak interactions. They also begin to reveal more detailed features, like the relative shortness of the D^0 lifetime, which has aroused considerable theoretical interest (see Rosen, *Phys. Rev. Letters* 44, 4; 1980; Bander *et al.*, *ibid.* 44, 7; 1980). Hopefully, this is only the beginning. □

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Dendrochronology

from Sara Champion

CURRENT European research in dendrochronology is extending the possibility of accurate dating back into the 1st millennium B.C., and in doing so is confirming observations which could previously only have been made by the conventional method of artefact cross-dating.

When Bruno Huber, the founder of European dendrochronology, died in 1969, his master chronology for Central European oak stretched back from the present for over 1000 years (Huber, *Handbuch der Mikroskopie in der Technik* 5, 171; 1970). He had shown that the considerable variations in the ring widths of coniferous trees in North America caused by extreme temperatures, which allowed the crossmatching of skeleton plots, did not occur in the tree population in the more temperate European climate, rendering this method of crossmatching inapplicable. Concentrating on oak, the

most commonly used building timber and one suitable for interregional study, he developed the use of the agreement percentage and the fractional disagreement percentage for the evaluation of similarity of different plots. His use of the semi-logarithmic scale making the curves independent of the magnitude of absolute growth facilitated the visual comparison of different sequences. His emphasis on the recognition of *indicators* and *signatures* resulted in the establishment of several important ones: the peak in A.D. 1421, the minima in A.D. 1311, 1320 and 1326, and in particular the A.D. 1530-1540 'saw', a sequence of five alternate rises and falls which has been matched in Hollstein's West German oak chronology and has been recognised even in Becker's silver fir sequence.

Recent work has concentrated on improving the procedure for the synchronisation of individual curves, (Eckstein & Bauch *Fortwissenschaftliche Centralblatt* 88, 230; 1969; Baillie & Pilcher *Tree Ring Bulletin* 33, 7; 1973) and on extending the existing chronologies both geographically and temporally.

Chronological sequences have been constructed using the widths of the rings as a basis of comparison. For periods where sequencing of growth rings yield inadequate information for cross-matching, it has been suggested that the density of the wood or the dimensions of the cells may more clearly differentiate the years. The use of X-ray densitometry for establishing the proportion of low and high density wood (Fletcher and Hughes, *Bull. of the Fac. of Forestry*, Univ. of British Columbia 7, 41; 1970; Hughes and Sardinha, *J. Microsc.* 104/1, 91; 1975), has particular significance for dendroclimatology, since it yields detailed information on climatic conditions, but also serves to distinguish individual growth rings more clearly than simple ring width. Density is affected, however, not only by climate, but also by soil type, position of the sample in the tree, and by species so much further research is required if density is to be used alongside ring width for establishing chronologies. Variations in the dimensions of wood cells from year to year also yield detailed climatological information, and may in the future be suitable for use in constructing chronologies.

The identification of a felling year is straightforward if the sample still retains all the sapwood and bark, but research is showing that the approximate felling year can be calculated if the transition between heartwood and sapwood is present, even if most of the latter has been removed because it rots more quickly. Researchers have produced different estimates however, for the number of years to be

allowed between transition and final ring. Recent work on the timespan between felling and construction, based on the examination of timbers for drying fissures, has shown that generally wood used in buildings, bridges and other structures was not seasoned before use (as opposed to wood for cabinet-making, panels etc.), and that construction year can be assumed to be within a year of felling, apart from obvious cases of re-used old timber.

Many centres in Europe have concentrated on constructing regional chronologies, and on extending them back into early history and prehistory. Hollstein's work at Trier (summarised in *Brit. Arch. Reports* 551, 33; 1978) has aimed at establishing a chronology valid for West Germany, Luxembourg and parts of France and Switzerland. This master sequence is closely comparable to Huber's sequence (the 1530-1540 'saw' has been recognised in over 100 cases), but stretches back to A.D. 822 without serious problems. Although there are gaps in the 8th and 4th centuries A.D., use of historical dates such as the construction of the bridge over the Rhine at Cologne in A.D. 310 has allowed Hollstein to extend his chronology back to the 8th century B.C. Differences in elevation, soil and climate between Central and Western Germany and the North German lowlands have necessitated the construction of local sequences, many of which run from the 10th century A.D. to the present. Two chronologies for Sweden extend back to the 6th and 9th centuries A.D., one for Denmark to the 14th (with an earlier section 694-1191), and one for Schleswig-Holstein to the 5th century A.D.

In the Netherlands Bauch (*Brit. Arch. Reports* 551, 133; 1978) and others have established two non-matching chronologies for oak. Their chronology I (continuous back to 1385 A.D., earlier section 1036-1325 A.D.) can be synchronised with the Hollstein and Huber sequences, but their chronology II (1109-1637±1 A.D.), derived from panel paintings and sculptures executed in the Amsterdam and Leiden regions, bears little relation to these mainland European sequences. It has recently been matched, however, with the Type A chronology established for South-East England, with a high agreement percentage for the period 1354-1588 A.D. The import of timber from England is unlikely and there are, anyway, differences in the quality of the panels so the similarities in the curves have been attributed to like patterns of slow growth. A problem remains in that no examples of the Netherlands chronology II curves have been found apart from the panels; it is suggested therefore that the timber came from a source completely exhausted by 1650, the date from which Dutch panels always show the ring pattern of chronology I.

In England, the two main types of chronology identified, A and H, can be

related to continental sequences. Type A, found in South-East England and Yorkshire, has strong similarities to the Netherlands chronology II, while Type H, found in the West and on higher ground, can be correlated with the Huber and Hollstein sequences. Fletcher (*Brit. Arch. Reports* 551, 154; 1978) suggests that there may be a connection between the two chronology types and the two species of oak, sessile and pedunculate, and their preferred environment; current research on species identification should illuminate this problem.

Species other than oak are under investigation. The silver fir has been shown not only to share signatures with oak (the 1530-1540 'saw', for example), but also to be uniform in its growth pattern over a large area from Czechoslovakia to the Vosges. Hollstein's extension of the secure 1100-year fir chronology back to the 7th century B.C. through finds at the Magdalenenberg, a tumulus in the Black Forest, has enhanced the potential of this species. A chronology for beech runs back to the 14th century A.D. in Germany; research continues on the more problematical series for spruce, pine, ash and larch.

The significance of this work for archaeology is crucial. More than 300 dated samples from the important Viking trading centre at Haithabu, Schleswig (Eckstein and Liese, *Germania* 49, 155; 1971) have allowed a detailed constructional history of the site to be made: building dates for houses, wells and pavements span a period of 118 years, and evidence for sequence of structures and re-use of timbers has been produced which would have been unobtainable stratigraphically.

Uncertainties over the correct calibration of radiocarbon dates and variability in measurement have resulted in an extreme reluctance to use this dating technique in Europe for sites of the 1st millennium B.C. Dendrochronology is now producing absolute dates for sites previously dated by relative methods. Excavations at the Magdalenenberg produced timber from the primary and several secondary graves. Hollstein dated the primary grave to 577 B.C., and the secondaries to between that date and 550 B.C.; on archaeological grounds the graves would have been dated to 600-550 B.C. A rich grave at Altrier, Luxemburg, dated archaeologically to 475-400 B.C., produced a *terminus post quem* of 473 B.C. for a piece of timber with no heartwood/sapwood transition present, and a felling date of 461 B.C. for a piece of charcoal found with the cremation.

These dendrochronological dates have confirmed those suggested on archaeological grounds; once the many floating chronologies for material of the 2nd, 3rd and 4th millennia B.C., currently 'dated' by radiocarbon, are fitted to the master curves, the earlier phases of European prehistory may become dateable in calendar years, causing perhaps some upsets to established archaeological sequences. □

SPRING BOOKS SUPPLEMENT

This above all...

G.S. Stent

THIS is the second of a series of books commissioned by the Alfred P. Sloan Foundation in which according to its President "a representative selection of accomplished and articulate scientists [are] to set down their own accounts of their lives in science", in order to further the public understanding of science. Who could be more fitting for such a commission than Sir Peter Medawar, supreme among contemporary scientists for his combination of scientific excellence, erudition and literary skill? But rather than simply composing another scientific autobiography, Medawar has "tried to write the kind of book I myself should have liked to have read when I began research before most of my readers were born": a young scientist's vade-mecum that offers succour and savvy to the innocent, as provided by an experienced, wise old owl. The scope of Medawar's advice is indicated by the titles of his 12 brief chapters: "How can I tell if I am cut out to be a scientific research worker?"; "What shall I do research on?"; "How can I equip myself to be a scientist or a better one?"; "Sexism and racism in science"; "Aspects of scientific life and manners"; "Of younger and older scientists"; "Presentations"; "Experiment and discovery"; "Prizes and rewards"; "The scientific process"; and "Scientific meliorism versus scientific messianism".

There is certainly a great need for a book from which young persons interested in a scientific career but lacking direct access to senior scientists can get some authentic ideas of what it's really like to be in the business. In fact, why hasn't it been done before? And Medawar, who as an adviser to the young sees himself as the Polonius, the Lord Chesterfield or the William Cobbett of science, goes some considerable way towards filling that need. Above all, Medawar emerges from these pages as a role model worth emulating for any aspiring youth: urbane, sensible, open-

Advice to a Young Scientist. By P.B. Medawar. Pp.109. (Harper and Row: New York and London, 1980.) \$8.95, £4.95.

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minded and liberal (but, of course, intolerant of nonsense), universally esteemed, and, most importantly, outstandingly successful, having reached the absolute pinnacle of his profession with perfect grace, never clawing his way to the top. Moreover, Medawar provides many *aperçus* of the scientific life, such as that "no working scientist ever thinks of himself as old, and so long as health, rules of retirement, and fortune allow him to continue with research, he enjoys the young scientist's privilege of feeling himself born anew every morning"; that "luck plays a real part in scientific research"; and that "the husband of a [woman] scientist must not expect to find *gigot de poulette cuit à la vapeur de Marjolaine* ready on the table when he gets

home from work probably less taxing than his wife's".

But I do not think the book completely achieves its aims. First, the down-to-earth questions posed in some of the chapter titles are not all given very helpful answers. Thus if the young reader really wants to know if he is cut out to be a scientific research worker, he is provided with little more on which to base his decision than the information that science (just like, say, professional skiing) is a very demanding and sometimes exhausting, albeit exciting and rather passionate, occupation, for which old-fashioned virtues such as diligence, a sense of purpose, and the power to concentrate and persevere are required. Moreover, he should be able to pass an intelligence test provided by Medawar, which consists of discovering what is wrong with the theory that El Greco painted unnaturally tall and thin human figures because of a defect in the painter's vision that made him see people that way. And if the young reader really wants to know what he should do research on, he is given little more guidance than the aphorism that he "who wants to make important discoveries must study important problems". By way of an example of an unimportant piece of research *not* worth doing, Medawar mentions the case of a young zoology graduate student who decided to find out why 36% of sea urchin eggs have a tiny black spot on them. It so happens that I myself am very interested in the role of topographic differentiation of the egg in embryonic development and became very excited on learning that there are black spots on a substantial minority of sea urchin eggs. In fact, I had already worked out the beginnings of a black spot theory of development before I came to the dénouement when Medawar reveals that there are no such black spots and that it was all made up by Lord Zuckerman. And if the young reader wants to know how to equip

Sir Peter Medawar when a graduate student at Oxford, 1936.

Ramsey and Muspratt, Oxford.

himself to be a scientist, he is informed only that he must read the literature (but not too much), that he better get some results (even if they are not original), that he should not waste time building equipment (if it can be store-bought), and, above all, that he ought to practise what Medawar in an earlier book called "The Art of the Soluble" (which is "making a problem soluble by finding out ways of getting at it — soft underbellies and the like").

A second shortcoming of the book likely to trouble intellectually alert young readers is a noticeable lack of logical consistency in some of the propositions put forward by Medawar. For instance, he cautions against "citing Marie Curie as evidence that women can do well in science; any such tendency to generalize from isolated instances will convince no one that they have a natural aptitude for science — it is not Madame Curie but tens of thousands of women gainfully and often happily engaged in scientific pursuits who should be called in evidence". Yet in the same chapter, Medawar merely cites the names of ten brilliant contemporary Hungarians (most of Jewish descent) to demolish the inference drawn from IQ tests administered at Ellis Island before the First World War "that 83 percent of the Jews and 80 percent of the Hungarians seeking entry were feeble-minded". Rather than generalizing from isolated instances, should Medawar not have called into evidence the tens of thousands of Hungarian and Jewish immigrants who did just about as well in the New World as their Anglo-Saxon fellow-citizens? Further, in one place Medawar indignantly rejects the notion that there are any national differences in the character of science — he mentions Japan in particular as one nation whose science has been falsely held to be somehow different from that of the West. In fact, he thinks that such "regional differences are intrinsically unlikely for methodological reasons" and asserts "that no experienced scientist seriously believes that they exist", thus sweeping away with a few strokes of the pen all of Joseph Needham's studies of Chinese science that show that culture *has* a profound influence on the way science is done. Yet a few pages later he makes fun of Fernand Braudel's dictum that history devours the present by calling it (with undoubted chauvinist irony) one of "those profound French epigrams, you know". In another place Medawar declares that "most of the very greatest scientists lived long before Alfred Nobel . . . founded the prize". But some pages later he declares that science has no limits and "will dry up only if scientists lose or fail to exercise the power or incentive to imagine what the truth might be". So, since the vast majority of scientists that ever lived have lived in this century and since their labours have presumably carried us far away from nineteenth century science into the limitless sea of scientific knowledge, the keen young reader will

Sir Peter Medawar.

wonder how it is possible that most of the "greatest" scientists happened to have been among that tiny band of old-timers. Is Medawar not giving himself over to what he castigates at the very end of his book as "Arcadian thinking [which] looks not forward nor far away but backward to a golden age that could yet return"?

In this connection it is instructive to examine Medawar's statement that "one can envisage an end of science no more readily than one can envisage an end of imaginative literature or the fine arts". Interpreted literally this statement would merely assert that it is just as easy to envisage an end of literature and the arts as it is to envisage an end to science. But in the context in which it appears in this book the intended meaning of the statement is undoubtedly the claim that it is very difficult to envisage an end to either of these activities. But this claim would be counterfactual since it is, in fact, easy to envisage an end of the sciences and the arts, as was done, for instance, by Hermann Hesse in his *Glass Bead Game*. And it is precisely from envisaging an end of imaginative literature and the fine arts in

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our time that the view that most of the very greatest artists lived before our time gains its logical justification.

But these niggling criticisms are not meant to imply that *Advice to a Young Scientist* cannot be read with great profit by young and old alike. On the contrary, the impression that I want to leave is that this book is bound to enlarge any reader's intellectual horizon and give him a good grasp of what it is like to devote one's life to what Medawar calls "exploratory activities of which the purpose is to come to a better understanding of the natural world". For such a life a critical mind is required, which Medawar challenges with the following "chestnut of immemorial origin":

Psychiatrist: Why do you flail your arms around like that?

Patient: To keep the wild elephants at bay.

Psychiatrist: But there aren't any wild elephants here.

Patient: That's right. Effective, isn't it?

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Re-opening the case for natural theology

Stanley L. Jaki

Creation and the World of Science: The Bampton Lectures, 1978. By A. R. Peacocke. (Clarendon/Oxford University Press: Oxford, 1979.) £10.

A THAW has been developing for the past decade or so in the icy truce between science and Christian religion. Such is the first thesis of the Reverend Dr A. R. Peacocke, Dean of Clare College, Cambridge, in this book, the greatly expanded form of the Bampton Lectures for 1978. Of course, there were some tell-tale events well antedating the 1970s. It was a quarter of a century ago that Julian Huxley chose to drop from the second edition of his *Religion without Revelation* a crusading preface, his clarion call to all men to rid themselves, once and for all, of the illusion of being dependent on a personal God. Such a call is almost invariably orchestrated in that major key which is the presumed irreconcilability of science and religion and which is aimed primarily at the Christian creed, the major form of belief in God in the Western world, the cradle and powerhouse of science.

As to the past decade, it has widely witnessed the survival value of religion without revelation, after the late Jacob Bronowski refused to heed a suggestion of BBC Television to let them produce his *Ascent of Man* without its anti-Christian innuendos. Religion without revelation could, if the second Bronowski-lecturer (Philip Morrison) was right, be developed by termites, who, given enough time, perhaps billions of years, would come up, so he claimed, with a telescope worthy of Palomar. Like-minded pundits are no longer the exclusive voice of the world of science. Indeed, not since the days of a 'holy wedlock' between science and Christian religion (the days of Boyle and Newton) has it been as fashionable for scientists to refer to creation as is the case nowadays. Peacocke avoids following them into taking the 2.7°K cosmic background radiation for a measuring device of time elapsed since the moment of creation. At any rate, cosmology (the basic science, if all science is cosmology — a proposition endorsed by Popper and with no touch of falsifiability) is not among the major sources, surveyed by Peacocke, of the thaw in question. He rather emphasizes the rising awareness among scientists of the revisability and socio-psychological conditioning of their assumptions and theories. Another major source is that

ecological concern which forced the scientific community to face up to questions of ethics, often germane to a theological perspective.

Therefore the times are propitious, Peacocke argues, for re-opening the case for natural theology. Indeed, its thorough recasting is in order, if it is true, and Peacocke, a biochemist by first training, has no doubt that science has clarified all basic questions about all forms of life, including man, by showing that all are but "living matter", so many complex agglomerates of atoms and molecules. Thus, if for the Christian theologian the world is to remain a "launching pad" towards its Creator, the world must be perceived through lenses "ground and polished by science" (page 48).

Peacocke does not have in mind a stance of "genuflecting to science", a stance anyhow incompatible with the stipulations of the Reverend John Bampton, Canon of Salisbury. The Bampton-lecturer, a scholar in Anglican orders, must preach "eight Divinity Lecture Sermons" on any of six specified topics, all of which restrict genuflection before the Triune God alone. Since the last of those topics, "the Articles of the Christian Faith, as Comprehended in the Apostles' and Nicene Creed", rests on the dogma of the createdness of all things visible and invisible, a thorough confrontation of science and Christian faith is unavoidable, if the latter is to remain a "reasoned service", a stipulation of the Apostle Paul. Such a confrontation is meaningful only if the merits of the respective claims of science and of (Christian) religion are subjected to a thorough scrutiny, which touches not only on the very foundations of Christian theology but also on those of science. Therefore an undertaking of this sort must be of considerable interest to those natural-scientist readers of *Nature* who cherish vistas of science extending far beyond their often extremely circumscribed specializations.

The theme of thaw and its background, which is the topic of Chapter 1, is followed by "Cosmos, Man and Creation" in which the shift from the world picture of classical physics to that of modern physics, the interplay of chance and necessity, and the Judeo-Christian doctrine of creation are dealt with. Chance is again considered in Chapter 3, and with special attention to the ideas of Monod, Prigogine and Eigen on emergent systems. Peacocke's discussion of the mind-body relationship, the substance of Chapter 4, may have come more logically after his discussion of evolution ("The Selfish Gene and What Men Live By"). All these chapters certainly should keep alive the interest of scientists.

Chapter 5, in itself a small book, is strongly theological, which is not to say that it is uninteresting. Peacocke's treatment of man's ethical nature and of Jesus' life and resurrection will delight those who savour process theology taken in

a broader sense. Peacocke explicitly dissociates himself from neo-Thomists and Barthians. His predilection is rather with process theologians who showed marked interest in science and who published mostly during the past decade. While attention to the latest is certainly a great merit, it may also trap one in what Duhem memorably described as the "gossip of the moment", hardly ever the harbinger of depth. Is it really a sign of profundity to give, as Peacocke does, so much and sympathetic attention to a recent interpretation of "Yahweh" as meaning "He who makes things happen" and ignore at the same time its long-standing interpretation as Existence Itself, an interpretation that played a crucial role in the history of Western thought? At any rate, if the provenance of "Yahweh" must be sought in the political experience of the people, the question arises as to why other Semitic tribes, equally battered, failed to formulate it? Peacocke does not face up to this question, nor does he take a recourse to revelation, which incidentally, is mentioned only once throughout the book. A curious feature, because the last two chapters, on ecology and hope, respectively, are pivoted on the uniqueness of Jesus which Peacocke portrays over half a dozen, not too impressive pages, and because it is his stated view that in his lectures "there are involved . . . a sequence of theological themes . . . that echo credal phrases from the Book of Common Prayer" (page ix).

Those phrases, from creation through redemption to final consummation, take on, as articulated by Peacocke, a distinctly phenomenological ring. This is not to suggest even the slightest doubt about his deep commitment to the Creed. But no doubts could be raised on that score about Henry Longueville Mansel, author of *The Limits of Religious Thought*, the Bampton Lectures for 1858. Yet, an Oxford don, apparently of some stature, was heard to remark: "I had not expected to live to hear atheism preached from the pulpit of the University" (see V. F. Storr, *The Development of English Theology in the Nineteenth Century 1800-1860*; Longmans, Green: London, 1913; page 422). Not that Mansel would have wanted to give the slightest comfort to atheists, or to anticipate a religion without revelation. But as that perceptive don realized — and long before the inner logic of Mansel's highly applauded lectures dawned on all — the premises on which he built could not accommodate a reasoned account of Christian revelation. Classic illustrations of the inexorable force of that logic can be gathered not only from theology and philosophy, but also from the sciences. The road is straight from Descartes to De la Mettrie, from Luther to Schleiermacher, from the early Copenhagen school to the multiworld-theory and the Tao of physics.

An ominous-looking logic may raise its head out of Peacocke's position on the

mind-brain relationship, an inevitable consequence of his unqualified endorsement of the philosophy of emergent systems based on the work of Prigogine and others. To throw a strong light on this possibility, it may not be amiss to recall a remark which Herbert Feigl, the chief modern articulator of the identity theory of the mind-body relationship, made to the author of a book in which the case for dualism is unabashedly argued throughout: "Of course, I could not disagree more with you. Unfortunately I cannot do even that when reading some professedly Christian theologians on mind and body. I am unable to comprehend their contention that their position is different from mine". Needless to say, Feigl's position is compatible only with a religion without revelation as meant by Huxley. As to dualism, which Peacocke considers only in the perspectives of demonology and witchcraft, it is, in his eyes, an invitation to physicalist reductionism. This is certainly true of that parody of dualism produced by Descartes, a point not specified by Peacocke, who fails to mention even passing other formulations of dualism. He opts for the identity theory, which he introduces with appreciative words on materialism and monism. His sole effort to retain the mind (soul) is a brief reference to the conceptual irreducibility of mental experiences to empirical parameters.

This is hardly enough, as would be noted by those taking a long look both in Pusey Street and South Parks Road, respective symbols, in Peacocke's phrasing, of theological and scientific strongholds. They would point out that to discourse on an issue so crucial both to revealed religion and to religion without revelation as is the true nature of the mind-brain relationship, it is not enough "to note from the stands certain aspects of the state of the play". Peacocke states in the same breath: "I would not presume to enter the lists of that fearsome tiltyard from which so many, more competent and distinguished philosophically than I am, have retreated to lick their logical wounds" (page 128). Wounds cannot be escaped as long as one truly joins the battle for truth. At most they can be kept disinfected by making one's position clear and thoroughly considered.

The absence of these two qualities undercuts time and again Peacocke's efforts. A subject like creation and the world of science brings one into encounter at every turn with that philosophy which cannot be done justice with the evasive remark that the case for a qualified realism, adopted in these lectures, "cannot be presented here" (page 22). This transparent tactic is in sight again after a long section on time as perceived in science (evolution) and in theology (eschatology): "The philosophical discussion of time is beyond our present scope" (page 334). Even natural-scientist readers, not overly sensitive to philosophical rigour, may feel uneasy on finding Peacocke declare in the

next-to-last chapter that he had until then used the word nature "without any attempt to define it". After all, he himself tells his readers that several decades ago two scholars listed no less than sixty-six meanings of "nature", a number that could since then only increase.

A perhaps trivial, but telling sign that Nature is not static. Of this, Peacocke's book is a spirited and well-informed reminder. But in order to mean anything, Nature, and certainly man's nature, must retain some identity across the welter of change, especially if change is a true growth. Had Peacocke considered this basic philosophical issue, the touchstone of the truth of realism, with the seriousness it demands, his never-dull effort to portray

the dynamism of world and man as "being-in-God" would have secured more persuasiveness for *pan-en-theism*, the label by which he wants his message to be known. The proofs of his book may have been read with unusual speed, though, undoubtedly, not with an eye on Canon Bampton's final stipulation: "the Preacher shall not be paid, nor be entitled to the revenue, before his sermons are printed." □

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Lives of the stars

J.H. Manley

The Uranium People. By L.M. Libby. Pp. 384. (Crane, Russak/Scribner's: New York, 1979.) \$15.95. To be published in the UK in May 1980 by Adam Hilger, at £8.50.

It is easier to convey what this book is not than what it is. It is not a novel though it would be better and less offensive if it were. It is not in the category of memoirs, for although there are personal recollections, there is also a liberal use of the personal accounts of others. The reader will find the distinction between these two sources very un-sharp and will slowly learn that the geographical chapter headings have more to do with the subject treated than the location of the author. It is not a historical work in spite of the impression conveyed by the inclusion of references. Even these are inconsistent

with a historical work. Some quotes are unreferenced; practically none to official papers and compilations are included. In addition one is aware throughout of the intrusion of the author's opinions and tastes in people and events rather than the balanced pro and con of the careful historian or the insightful analysis of the biographer. The fact that the book grew out of a series of lectures as a university visitor suggests that the material and style were meant to be entertaining to students. I believe they were. There are many anecdotes, some amusing, some instructive, some flat. The author's statement of her purpose is to "tell not only what was scientifically interesting about the uranium-plutonium project but what was human and interesting about the people involved". Let us therefore consider her treatment of people and events.



Enrico Fermi (left) and John Marshall, 1946.

The title suggests an emphasis on people and the text starts that way. I confess to disappointment in Libby's efforts to describe people. Practically all the individuals for whom she implies a personal acquaintanceship are also known to me, some less well, some better, but I did not recognize them very well as portrayed. I finally concluded that at least part of the reason is her often tasteless and tactless choice of anecdotes used to suggest interest in so-and-so. There is a strained and misguided effort to make people seem more human by calling attention to uncommon mannerisms or recounting stories which are easily called crudities of various degree. Mention of an instance in which Laura Fermi listed the inner organs of a turkey says nothing about her graciousness. The worst crudity, an episode used to explain a facet of graduate student collective mores, involves the death of a colleague. To a point, foibles help to visualize a person and may make him interesting, but other less curious personality descriptions are needed for balance. These are missing, and even the non-anecdotal information is confined largely to physical characteristics. Rarely are adjectives relating to character used. In one case she explains that a person has been called a "delta-function of sympathy" but even an included definition of that term does not bring much of the person to the reader. In the sense that common gossip more frequently delights in communicating less desirable human traits as well as being idle, many of Libby's descriptions are gossipy. The less favour in which she holds a person the more gossipy the material. Leo Szilard as an individual is not crystallized by a good explanation of the Szilard-Chalmers effect. Fortunately the inclusion of a quote about him from Jacques Monod contributes excellently. Similarly, General Groves becomes more real through a quote from Emilio Segrè.

One could wish that at least a few of the people might have received the kind of thorough, thoughtful and sensitive treatment Nuell Pharr Davis achieved in *Lawrence and Oppenheimer* (Simon and Schuster: New York, 1968). The difference is all the more remarkable when one realizes that Libby lived and worked with many of her people, but Davis became acquainted only by interview, albeit with an impressive list of individuals. *The Uranium People* is a misleading title. It is more about events and stories of people, with little attempt to examine the humans at any length or the subtle reasons why they so frequently found each other interesting.

Events fare better in this book than people. Descriptions of places though not extensive are well written and convey correct images; so with technical explanations, some of which may not be essential to the lay reader. With few



From left, Ernest Lawrence, Enrico Fermi, I.I. Rabi at Los Alamos lodge.

exceptions I found no serious errors of historical or scientific fact. Use of many interesting memories of the author and of others makes a readable story. As with people, the interpretation of events is well tinted by the author's personal preferences and opinions.

Scientific-military-industrial roles in the project are discussed in several places, most frequently in terms of good scientists and bad military and industrial people, though not consistently so. One episode described contains speculation on a possible not-quite-right cosiness between General Groves and the engineering firm of Stone and Webster. This gets confusedly related to Arthur Compton and God, to a belief of a number of Metallurgical Laboratory scientists that industry need play only a minor role and to a jibe at Groves for depriving Compton of responsibility for weapon research and development in favour of Oppenheimer. It was surely naive of the scientists (including the author) to think that the production reactors and extraction facilities could be designed and built by them with a few hundred engineers and draftsmen. Libby implicitly acknowledges this in a later description of the DuPont achievement of completing this technical task including a city to house 60,000 construction workers all in 18 months. However, she has no appreciative word for Groves for his wise and effective choice of this contractor. The account of Groves' shift of weapon responsibility is quite wrong. After May 1942 Oppenheimer and I were sharing responsibility for the geographically scattered weapons work under Compton.

In a few months it became very clear that the work would be hopelessly behind unless it were centralized in a new laboratory with proper facilities. Compton concurred completely and fully supported our view to Groves. It was obvious that he could not carry the responsibility for two widely separated laboratories; a search for a Director was initiated and Oppenheimer chosen with complete approval of Compton. I felt he was glad not to have the weapon project.

The last chapters of the book cover such events as the establishment of the Atomic Energy Commission, the Russian A-bomb, and the H-bomb controversy and aftermath; and there is a chapter on atomic power. The style of writing continues, perhaps intensifies. Groves remains a bad guy until she can report his departure from the atomic scene with words symptomatic of her approach: "General Groves had put down Arthur Compton. Now his own boys, Robert Oppenheimer, Bush, and Conant had put him down. And soon, Oppenheimer was to be put down, in turn, by the Grey Board". Gentle, understanding, sympathetic! Libby finds it "hard to understand how the General Advisory Committee, chaired by Oppenheimer, could have persuaded itself to have voted negatively on the question of whether the United States should make an urgent programme to develop the thermonuclear bomb". At first one thinks that perhaps she did not pay enough attention at the time and then later failed to read Herbert York's careful and extensive examination of this and associated issues in *The*

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Advisors (Freeman: San Francisco, 1976). Further on, however, she has included a selective quote from him but not his conclusion that the advice of the Committee was "sound and right". She also writes: "One may ask how the GAC members, strongly opinionated scientists, could have so changed their vote on the hydrogen bomb in a couple of weeks". Reference is then made to a meeting on 29 October and then to a "second meeting". Actually, there was no change of vote, and the written views, unanimously against an all-out effort, are dated 30 October. If entertainment is the objective of Libby's book, perhaps these details are unimportant, but I view such distortions as poisonous. The student and the serious reader must take something like York's book as an antidote; then Libby's continued loyalty to "one of my heroes", Edward Teller, can be set in contrast to York's development from his early career as the first director (1952-57) of Teller's

Lawrence Livermore Laboratory to his subsequent activity in arms control and disarmament.

The Uranium People is interesting in the same sense that movie magazine stories about the stars are interesting. However, events and people, now for the most part passed into history but leaving a legacy which still affects our world, have a different kind of interest and deserve a different kind of treatment. □

J.H. Manley became active in nuclear physics at Columbia as a faculty member with I.I. Rabi, John Dunning and others in 1934, and joined the Metallurgical Laboratory at Chicago under A.H. Compton in January 1942 while on leave from Illinois. He assisted J.R. Oppenheimer in planning and establishing the Los Alamos Scientific Laboratory, to which he moved for what turned out to be a total of 23 years before becoming a consultant — his present status. He was Executive Secretary of the General Advisory Committee (USAEC) during the first four years of Oppenheimer's chairmanship.

Descent is what matters

David L. Hull

The Darwinian Revolution: Science Red in Tooth and Claw. By Michael Ruse. Pp.320. (University of Chicago Press: Chicago and London, UK, 1979.) £12.

MOVIE reviewers like so few movies because they see too many of them. For the same reason, academics write very few favourable reviews of books, especially if the book is in their own area and is directed at a general audience. It is difficult to believe that yet another book on Darwin and the Darwinian revolution could add anything new or contain any surprises. Ruse's book is an exception on all counts. Darwin scholars and the general reader alike can learn from it.

In the first half Ruse describes the state of science before the *Origin*. Historians argue that scientific views must be set out in their own terms, not biased towards the use which later scientists will make of them. That is easier said than done. Ruse cannot discuss every topic of interest to scientists in the first half of the nineteenth century. Understandably he limits himself to those areas of science which eventually contributed to Darwin's theory of evolution — Charles Lyell's uniformitarian geology, Richard Owen's archetype theory, Karl Ernst von Baer's embryology, and so on. In certain cases, Darwin incorporated these views into his own. Just as often his ideas were shaped by

reacting against the views of his predecessors. But, in any case, once Ruse has narrowed his focus, he discusses the science in its own right, not biased towards the use which Darwin eventually made of it. Although Ruse teases us a bit along the way, he does not get around to Darwin and his theory until the middle of the book. All the attention which Ruse pays to the scientific work which preceded Darwin, instead of detracting from the magnitude of Darwin's own contributions, only enhances it.

Another strength of Ruse's exposition is his concern with the impact that social factors had on both the formulation and the reception of Darwin's theory. However, the 'society' of greatest interest to Ruse is not Victorian society at large but the society of scientists. Darwin was caught up in the groundswell as science in Great Britain became self-consciously a profession. Young scientists today can only envy the ease with which budding young scientists in Darwin's day were able to step into the innermost circles of science. To begin with, relatively prosperous Victorians were a tightly-knit group. For example, the first three directors of Kew Gardens were William Hooker, his son J.D. Hooker, and William Thistlethorn-Dyer, the younger Hooker's son-in-law. J.S. Henslow was the first scientist to have significant influence on Darwin, and J.D. Hooker was his first convert to evolutionism. Henslow married the sister of Leonard Jenyns, the man who had been offered the position of *de facto* naturalist on the *Beagle* before Darwin. Their daughter in turn married J.D. Hooker. Upon her death, Hooker married the widow of William Jardine, a famous ornithologist. Darwin married a Wedgwood,

and so it goes. Of equal importance, so few young men were inclined or financially able to contemplate a life of science that the ones who did were welcomed enthusiastically.

Darwin began his career as a Lyellian geologist. In retrospect we tend to think that Lyellian uniformitarian geology was more successful than it actually was because the first histories of geology were written by uniformitarians. To the victors goes the history. But in fact Darwin was one of the few young geologists at the time who accepted Lyell's views. Darwin's early work on the formation of coral reefs (which turned out to be basically correct) and the parallel roads of Glen Roy (an embarrassing blunder) was in the Lyellian mould. Gradually, however, Darwin became his own scientist, departing in several important respects from Lyell. By 1859 not only was Darwin a professional scientist with a well established reputation, but also he had "built around himself a new scientific community from which his evolutionism could be launched" (page 185).

One of the great puzzles in science is why certain scientists succeed in converting their fellow scientists when all their precursors failed. For example, Lamarck's theory was greeted with derision, Robert Chambers's *Vestiges of the Natural History of Creation* was condemned by everyone who was anyone, including T.H. Huxley, and Herbert Spencer's musings about the transformation of homogeneity into heterogeneity produced little more than embarrassed silence. Ruse gives many reasons for Darwin's success — such general considerations as the changes wrought by the industrial revolution and the rise of naturalistic metaphysics to such specific considerations as the way in which Lyell posed the species problem in his *Principles of Geology* — but of prime importance was the professional manner in which Darwin conducted himself and presented his case. Lamarck had destroyed his credibility by tossing out theory after theory in areas in which he knew nothing; Chambers was the Velikovsky of his day; and Spencer was a "philosopher". But Darwin was a scientist, writing as a scientist, for scientists. Nineteenth-century views on the nature of science had as much to do with the reception of Darwin's theory as religion and the fossil record.

The most entertaining part of Ruse's exposition is the attention he pays to the interpersonal relations between the scientists of the period. Although Owen initially helped the young Huxley in his career, he soon came to be Huxley's chief enemy. Because the Darwinians won, everyone knows what an unpleasant man Owen was, but Huxley too had a mean streak. After Owen tried to block the publication of one of Huxley's papers, Huxley used his review of the *Vestiges* to attack him. As Ruse sees it, the animosity between Huxley and Owen was as much

responsible for Owen coming out against Darwin as the content of Darwin's theory. "At the scientific level Owen was determined to oppose Huxley, and if Huxley was to support Darwinism then so be it" (page 144). Ruse also mentions, possibly to neutralize anti-Owen propaganda, that "at least until 1853 — when Huxley started attacking Owen and when Darwin and Huxley became close — there was warm feeling between Darwin and Owen" (page 144). The source of this claim is the following remark which Darwin made in a letter to Huxley in 1854 about Huxley's review of the *Vestiges*:

By Heavens, how the blood must have gushed into the capillaries when a certain great man (whom with all his faults I cannot help liking) read it!

This is warmth of sorts.

One of the chief messages of evolutionary theory as a biological theory is that descent is what matters, not abstract similarity. Ruse applies this same principle to the history of science. No scientist has suffered more from the effects of the antiquarian disease known as "precursoritis" than has Darwin. Endless energy has been expended to uncover precursor after precursor — authors who happened to mention in passing one or more views similar to those later enunciated by Darwin. More recent historians have complained that often the putative similarity is spurious. But the important point is that ineffectual precursors do not count. As Ruse argues, "it would be a mistake to think that the most significant intellectual continuity in the Darwinian Revolution existed among those one would label 'evolutionist'" (page 199). The real precursors to Darwin were not Lamarck, Erasmus Darwin, William Wells, Patrick Matthew and Edward Blyth. Darwin was totally unaware of the work of several of these men, and the others had little effect on him or anyone else. Darwin's real precursors were Lyell, Henslow, Owen, Herschel, Whewell, von Baer and Sedgwick. These were the men who contributed to the Darwinian revolution. What counts in the history of science is continuity of effect, not similarity of ideas. Likewise, as Ruse remarks, a "Darwinian" is "someone who identified with Darwin but not necessarily someone who accepted all of Darwin's ideas" (page 203).

During the past decade, Darwin scholars have increased our understanding of Darwin and the Darwinian revolution immensely. Ruse gives his readers the benefit of that scholarship without making them suffer under the burden of the tedious pedantry which usually accompanies it. □

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Full of hope and promise

R. V. Short

The Politics of Contraception. By Carl Djerassi. Pp.274. (W.W. Norton: London, 1980.) £6.50.

CARL DJERASSI, one of the world's outstanding organic chemists, has written a most important book about contraception that makes compelling reading. It deserves the widest possible audience, not only throughout the scientific community, but far beyond, in the realms of policy-makers, politicians, journalists, administrators, funding agencies, and indeed amongst all who are concerned about world futures. There are few indeed who have mastered the art of popularizing important issues without seeming to trivialize them; this book stands out as a rare example of scientific reporting at its very best. Djerassi's writing reflects the man himself — forthright, scholarly, direct, uncompromising, and with an outstanding ability to single out important issues for critical comment. He shares with Samuel Pepys "a fine conceit" that adds to, rather than detracts from the narrative, because one respects the integrity and intellect of the man behind the pen.

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Carl Djerassi

Djerassi tells us that he has aimed to produce "not a scholarly treatise, but the distillate of the conclusions and opinions of a person who for over 20 years has lived a bigamous professional life in serving simultaneously as a professor carrying out basic research and as an industrialist who has had to concern himself with finding worldwide applications for laboratory discoveries". Not only has he been able to combine the viewpoints of a professor of organic chemistry with those of a director of a pharmaceutical company, but he has added profound biological and social insights; it is to this amalgam of talents that we must attribute his success as an

international entrepreneur, and the wide appeal of this particular book.

Djerassi will long be remembered for an article he wrote in *Science* in 1970 (*Science* 169, 941-951) entitled "Birth control after 1984", and his book is an extension and development of the ideas put forward in that article. The need to redouble our efforts in contraceptive research and development is self-evident at a time when the world's population is still continuing to increase by about 150,000 people a day; this single fact still continues to pose the greatest threat to the peaceful co-existence of mankind in the future.

Our principal goal must be to develop new approaches to contraception capable of deployment on a massive scale to meet the needs of the developing world; of relatively minor importance is the improvement of methods that we already have in the West, so as to maximize their efficacy and minimize their attendant health hazards. But unfortunately it is our obsession with these latter objectives in developed countries that has seriously impeded research into new contraceptives. Much of this research must inevitably be carried out in the West, because it is only in the developed countries that we have all the necessary facilities, funds, and trained minds, together with the industrial know-how to exploit the discoveries. Herein lies the dilemma. In the first place, the declining rate of population growth in Western nations has allayed public and political concern for the problem, so that governmental support for research has now switched to more immediate problems such as alternative energy sources. This change in emphasis has coincided with the development of a plethora of governmental regulations about drug safety, and the establishment of a bureaucracy to enforce them. The statutory requirements for long-term animal toxicity testing before a drug can be marketed means that it may take from 10 to 17 years between the discovery of a compound, and its approval for clinical use by the regulatory agencies. Thus it is hardly surprising that new product development in the contraceptive field has become one of the least attractive areas of research for the pharmaceutical industry; but without the active involvement of industry, our hopes of any significant new contraceptive developments are faint indeed.

In our national drug regulatory agencies we have created organizations who have absolute power, but no sense of responsibility to see that a compound that is desperately needed is ultimately marketed; witness the status of the injectable contraceptive Provera. Surely we need to establish some mechanism whereby the pharmaceutical industry and the regulatory agencies could have a common cause once it has been agreed that there is a need to develop drugs to meet a particular demand. Under the present system, the regulatory agency acts as State

prosecutor, and private industry is the defendant. The case only comes to trial after years of time and effort and, as Djerassi points out, the pharmaceutical company does not have a friend in court. Surely there would be much to be gained by exchanging the trauma of a final examination for some type of continuous assessment?

Although agreeing with the need for governmental regulations about drug development and safety testing, Djerassi highlights the bureaucratic stupidity of some of the consequences. For example, the US Food and Drug Administration's requirements for condoms have meant that America produces the thickest condoms of any country in the world; if standards were relaxed to permit a 1% increase in the incidence of pin-holes, condom thickness could be reduced by half, and the increased risk of pregnancy resulting from this would only be one conception per 2.5-5 million acts of coitus. The instructions that now must accompany each packet of oral contraceptives are almost unintelligible even to the expert, and Djerassi contrasts this to the simple and effective wording that is used in China. Indeed, the chapter devoted to birth control in China is one of the most fascinating and challenging in the whole book.

Djerassi inevitably paints a rather gloomy picture about our ability to develop those new approaches to contraception which we so desperately need. But he may be unduly pessimistic, as he has based his strategies for the future on a logical planned approach of contraceptive research and development, and has ignored the major role that accident and chance can take in such affairs. For example, we can derive some comfort from the fact that in

reviewing probable future developments in the field, Djerassi does not even mention what to many seems to be the most exciting hope for the future, the use of analogues of gonatrophin releasing hormone. It has recently been shown that daily administration of microgram amounts of one of these peptides can effectively inhibit ovulation for long periods of time in monkeys and in women. Here we have an example of a serendipitous discovery; these readily synthesized, non-toxic, non-steroidal compounds were developed by the pharmaceutical industry for the treatment of some forms of infertility, but paradoxically they can also be used as contraceptives. As they have already passed many of the drug regulatory agencies' hurdles, and are on the market in some countries, they could be available for widespread clinical use within a few years from now.

So the future is full of hope and promise, provided that the West can increase its funding for contraceptive research, and remove the existing bureaucratic impediments to commercial development. But there are many obstacles to be overcome if we are to develop a rational approach to contraceptive research and development, and the world must be indebted to Carl Djerassi for pointing out some of the illogical constraints we have imposed upon ourselves. There can be no doubt as to the truth of his general conclusion that it is the politics of contraception, rather than the science, that now plays the dominant role in shaping the future. □

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The micro future?

Stuart Sutherland

The Mighty Micro. By C. Evans. Pp. 255. (Gollancz: London, 1979.) £5.50.

OVER the past 20 years, many voices have noisily if somewhat prematurely hailed the advent of the computer revolution. It has had no more enthusiastic advocate than the late Christopher Evans, whose premature death occurred after *The Mighty Micro* had been published. It has for long been customary to place the revolution about two decades after the date at which the prediction is made, and Dr Evans adheres to this convention. By AD 2000, we will have, according to him, universal electronic mail, greatly improved Prestel sets on our wrists, and permanently worn silicon chips to monitor our health and automatically inject corrective drugs as needed. Money will have been abolished

since all financial transactions will be computerized, and computers will administer psychotherapy and justice as well as taking over the function of the teacher: bookshops and libraries will no longer exist, since it will be possible to call up anything ever published on your drawing-room VDU.

The Mighty Micro is written for the general public, not for the specialist. It is lively and entertaining, but makes no attempt to describe how a silicon chip works nor does it provide any insights into the intricacies of computer software. It contains some good anecdotes about the forerunners and pioneers of computing — Babbage, Hollerith, Zuse and Schreyer (who developed special-purpose computers in Germany in the Second World War), Aitken, Turing and Von Neumann. The pioneers saw a visionary

future for computing, but without the invention of the silicon chip their visions could never have been realized, and that invention, as Dr Evans points out, could never have been foreseen. It transformed the cost, speed and reliability of computers, and made it practical to multiply the components almost without limit.

Most of *The Mighty Micro* is devoted to the future, and is speculative rather than closely argued. Dr Evans relies heavily on analogies, many of them striking. To give the reader an idea of what the exponential growth in computers portends, he points out that a piece of paper folded on itself 50 times would stretch beyond the orbit of Mars, a result that few would guess without calculation. The analogy may be better than he realizes: just as there are practical limits to such exponential growth in the thickness of a piece of paper, so there are invariably factors that set limits to the continuing exponential growth of a new technology.

Perhaps the most obvious such factor is the provision of the sort of software Dr Evans envisages. He is surely being unduly sanguine when he writes "There is no suggestion that before substantial improvement [in machine intelligence] can take place, any new principles must be invoked or new discoveries made". It is true that advances in artificial intelligence have over the last ten years thrown some light on how the human brain is likely to execute certain tasks, but the mirage of actually using intelligent programs to do something useful constantly recedes from us and there is still only a handful of applications. One reason is that very few general principles have been discovered, and it is necessary to work out *ab initio* the structure of each new intelligent task before attempting to program a computer to execute it. Hence, computers have so far proved to be an effective substitute for the human brain only in tasks, such as mass spectrometry and chess, the structure of which has been made explicit and which necessitate a search through a vast number of alternative possibilities. Where, as in economic decision taking, we have little understanding of the task, computers cannot carry it out unaided, and are probably more of a hindrance than a help to our own thinking if used as a tool, since the spurious authority of computer forecasts may make us forget the fallibility of the assumptions on which the forecasts are based. As for Dr Evans' claim that we will shortly be able to design a machine that is more intelligent than ourselves and which can in turn design machines more intelligent than itself, we have at the moment no inkling of how to set about such a problem; nor is there any evidence to suggest that it would ever be possible. There remains, then, an astounding gap between the prognostications of some practitioners of AI and the performance of existing programs, and there is no sign of

the impending discovery of a software equivalent of the silicon chip that will open the way to dramatic advances in the subject.

Although Dr Evans may have compressed the time scale, computers will inevitably change our living conditions; but his enthusiasm for the prospect will not be shared by everyone. He appears to think that the extra leisure time left when computers are performing most of both the important and the menial jobs can be filled by playing games against them: but people like to feel they are doing something useful and a life devoted to such games would be a hell of boredom. The Greeks, provided with leisure by slave labour, occupied themselves with making war on their neighbours, parochial politics and speculations about the nature of the Universe. We can no longer fill our time by making war, and if computers were ever able to take political decisions more sensibly than ourselves and to undertake advanced scientific work, it would come to seem pointless for us to engage in these activities. It is perhaps fortunate that in the lifetime of anyone now living we are unlikely to design our own intellectual successors, and hence render ourselves redundant. □

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Illusions of pedigree

J.D. Mollon

Seeing: Illusion, Brain and Mind. By J.P. Frisby. Pp.160. (Oxford University Press: Oxford, 1979.) £6.95.

THOSE who teach experimental psychology will instantly recognize the pedigree of this handsome colt: it is sired by Richard Gregory out of Lindsay and Norman. It lacks the turn of speed of its parents but proves an excellent stayer on soft ground, is usually reliable over hurdles and should bring rewards to its backers.

Professor Frisby is to be congratulated on an engaging and remarkably lucid introduction to the problems of visual perception. His declared purpose is to combine the psychological, physiological and computational approaches to perception. He has a good judgement of the important issues. Topics covered in detail include feature detection, figure-ground differentiation, the anatomy and physiology of the visual cortex, object recognition, the computation of lightness, and binocular stereopsis. The concept of spatial frequency and the contrast sensitivity function are deftly introduced for the novice reader. The book is sumptuously and skilfully illustrated and the figures allow readers to experience for themselves many illusions and perceptual after-effects; indeed the author recounts in his preface how he originally set out to produce a collection of illusions for the general reader but, like a novelist taken over by his characters, was soon led to provide the intellectual background. Two-colour anaglyphs, many of them original, demonstrate phenomena of binocular vision. The major omission is a discussion of colour vision; this is odd, even inexcusable, given the lavish facilities available for colour illustration.

The exposition is one of almost unflinching clarity and this is perhaps the most impressive quality of the book. If Frisby has any fault it is that of spelling points out too much — a virtue in a lecturer but a vice in an author; thus he devotes no less than ten, similar, figures to introducing the concept of a corner-detector.

In elaborating the computational approach to visual perception, Frisby acknowledges an explicit debt to D. Marr "whose writings are for me the work of genius". He discusses in detail, for example, Marr's theory of how the retina computes lightness, that is, discovers the reflectances of surfaces that are unevenly illuminated: bipolar cells detect edges by a centre-surround differencing operation; a threshold is applied to the resulting signal; and then (the "deconvolution" stage) lateral facilitation within one class of

ganglion cells causes all cells corresponding to points within a closed boundary to take on the lightness-signal indicated by the signal at the edge. Now, Marr's histological reification of this algorithm is, as Frisby says, "controversial" (to say the least), but there is a more general difficulty. The whole of Chapter 1 was devoted to disabusing the novice reader of a belief in the 'inner screen' theory of perception (the view sometimes called the Gestaltist fallacy); if our novice reader has taken that chapter to heart, then, when he reads Frisby's account of the computation of lightness, he will want to know why the visual system goes to such trouble to produce a picture of the scene in which lightnesses are redundantly represented in the interiors of homogeneous areas, why, that is, the deconvolution is necessary. And he won't be told.

Frisby leaves to a late stage in the book the details of retinal anatomy and physiology. Perhaps it is in this area that he is least sure of himself, for it is here alone

that errors occur in any density: in the course of two pages, the inner and outer synaptic layers are confounded, the pigment epithelium and choroid are labelled "sclera", rods are said to be 500 times more sensitive than cones because they contain rhodopsin, the fovea is said to contain only cones, an error of 10^3 is made in equating retinal extent to visual angle, and the amphibian mudpuppy (in a remark apparently lifted almost verbatim from Blakemore) is described as a fish. Undergraduates should be directed elsewhere for their retinal physiology.

The contributions of the editor, Henry Hardy, and the art director, David Warner, are so manifest in this book that their work deserves explicit congratulation. *Seeing* will be equally in place on the coffee table and on the first-year reading list. I strongly recommend it. □

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Flawed search

Sara Champion

In Search of Ancient Astronomies. Edited by E.C. Krupp. Pp.277. (Chatto & Windus: London, 1980.) £8.95.

THIS book arose from a series of lectures given by the contributors in California in 1975, and is, according to its editor, "the first attempt to present systematically to the general reader the main results of archaeo-astronomy to date". It consists of seven chapters, four by the editor and one each by Professor Thom and his son, Dr John Eddy and Dr Anthony Aveni. It is clearly stated in Krupp's introduction that the "pseudoscientific misconceptions" of Erich von Daniken, Velikovsky and the like are to be "dispelled by the reliable, scientific findings of archaeo-astronomy"; it is unfortunate, therefore, that the majority of the chapters show some misuse or misunderstanding of archaeological data and imprecision in the demonstration of astronomical alignments.

The first chapter by Krupp himself is presented as a non-mathematical introduction to practical astronomy. This is necessary for the understanding of subsequent chapters, and is in general comprehensible, though some of the information could have been transmitted with fewer words and greater clarity.

For many readers the work of the Thoms will be the most familiar, and their chapter on stone circles and menhirs follows. Thom's eminence in the field is largely based on the accuracy of his measurements and the precision of his analysis. Recent papers and reviews have, however, cast doubt on some of his figures, with serious

results for his megalithic yard and megalithic calendar (Moir *et al.* *Antiquity* LIV, 37-43, 1980). Misidentification of archaeological monuments has resulted in Thom's describing hut circles and enclosures as stone circles of megalithic type, and some of the lines of foresight suggested for certain sites are shown in the field to be below horizon level, difficult to see without binoculars or blocked by natural features not obvious from maps. Some controversial sites appear in this chapter: the Crucuno Rectangle, for example, described by Thom as "lunar standstill alignments", is claimed by Daniel in *Antiquity* XLIX, 81, 1975, to be an AD eighteenth century folly. For the archaeologist prepared to be convinced, some of Thom's claims appear thin and several statements contentious. To be told that, at Rough Tor in Cornwall, many of the upright stones have fallen and the remaining ones do not adhere closely to the flattened circle design, perhaps because of solifluxion, begs the question — what flattened circle design is there left to be measured. Rules about the use of megalithic yards are stated, only for exceptions to be made immediately: "the sides of these triangles all had to be integers in megalithic yards", followed by "the builders discarded the rule that all radii must be integers".

The third chapter, by Krupp, deals with the work by archaeo-astronomers and others on the site of Stonehenge, several of whose papers have appeared in *Nature*. In general the treatment is descriptive rather than critical, which may be misleading for the general reader. Analysis of Hoyle's claim that Stonehenge was an eclipse predictor has been published by Moir (*Antiquity* LIII, 124-128, 1979); he points

out the problems of refraction, visibility, and variability in moonrise and moonset positions due to declination and perturbation which would affect the use of the site in this way. The same criticisms can be applied to Thom's work where it involves lunar observations. Again, the misuse of archaeology undermines attempts to prove alignments: Krupp's use of Figsbury Ring, at least 1,000 years later in date than Stonehenge, as a foresight for the southern major standstill moonrise, is inexcusable.

Eddy's chapter on North America is the only one in the book where a sensible caution appears. His balanced view of others' work is exemplified by his regard for the archaeology as equally important as the astronomy in the analysis of the Chaco Canyon structures. His own work on the Indian medicine wheels is a model of careful and logical procedure. The same cannot be said of Aveni's chapter on

Mesoamerica, where sites a millennium and several hundred miles apart are grouped on the basis of their similar orientation. The use in this chapter of words like "nearly", "close to" and "approximate" when describing alignments destroys their credibility. The claimed alignment of Teotihuacán on the setting of the Pleiades turns out to be "within 1 degree" of this event, a difference representing a sizeable slice of the horizon. Misunderstanding of archaeological data in this chapter results in Aveni using Flannery and Marcus' application of central place theory to Mayan settlements as evidence of geometrical and possibly astronomical location of these sites.

Krupp's chapter on Egyptian astronomy, mainly descriptive of previous work, is followed by his attempt to debunk von Däniken and the rest. His handling of the leyline controversy and the Glaston-

bury Zodiac lacks conviction, since he frequently ignores the most obvious line of attack; for example, he reproduces without comment the notorious leyline that runs from Stonehenge (third millennium BC) through Old Sarum (sixth century BC) to Salisbury Cathedral (AD twelfth century).

With the exception of Eddy's chapter, the book fails on its own terms, for it presents to the reader a picture based, in many cases, on misunderstood archaeological evidence and selective or imprecise numerical and astronomical data. The non-expert may unfortunately be persuaded by the jaunty air and punning subheadings (e.g. "A Serious Mystery" for a section on Sirius) into believing it all. □

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FRS extraordinary

J. Z. Fullmer

Benjamin Thompson, Count Rumford. By Sanborn C. Brown. Pp.576. (MIT Press: Cambridge, Massachusetts, and London, UK, 1979.) \$19.95, £12.40.

BENJAMIN Thompson, born in March 1753 in Woburn, Massachusetts, died Benjamin Thompson, Count Rumford, in August 1814, in Auteuil, France. By almost any criteria his biography should make a rattling good yarn, for his adventurous life had episodes of intrigue, of mystery and of passion. He promoted large scale social experimentation and technological innovation; he interpreted his scientific experiments in ways that propelled him into controversy with the conservative members of the scientific community. When young and a volunteer member of the American army he functioned as a Royalist spy. In the spring of 1776, he fled to England, where he promptly ingratiated himself with influential British government officials. In 1783 he went to the Continent, becoming an agent for the British in Munich (or, perhaps, a double agent — the record is cloudy). Subsequently he shuttled back and forth between England, Ireland and Europe with no apparent regularity or design, his movements dictated by opportunity. He acknowledged two illegitimate children, one in Munich, the other in Paris, and sired one legitimate child, Sarah, born in Massachusetts. Assured that his abandoned American wife was truly dead, he married Jeanette Lavoisier (*née* Paulze), Antoine Lavoisier's widow, from whom he was soon divorced after a series of spectacular and public brawls. Moreover, during long

periods in England and on the Continent he associated closely with well-placed reputed homosexuals, furthering his own career through these connections.

He managed to be close to centres of power in the government circles to which he propelled himself, and in the international scientific community. In 1780 he was elected Fellow of the Royal Society — he was 27 — after having attracted favourable notice for his very long paper on the nature of heat. For nearly a decade he earned the attention of Karl Theodore, the Elector of Bavaria, to whom Rumford owed his honorary title. The scientifically conservative Napoleon showered attention on him, and in England Sir Joseph Banks, PRS, abetted some of his plans. Rumford published a large number of papers: on the nature of heat

and of light; on the best designs for stoves; on ways to feed the poor; on the efficient use of fuels in kitchens and fireplaces; on the boring of cannon; and on a pot for brewing coffee. He laid out the English Gardens in Munich; he devised plans to reduce beggary in Bavaria while feeding and clothing the army; he promoted the erection of a technological museum that offered visitors 'hands-on' experiences; he attempted to educate artisans in the factory ways of the industrial revolution; together with Banks he founded the justly famed Royal Institution of Great Britain.

Yet even this extensive listing of his promotional efforts does not exhaust them. While still alive Rumford proffered sums of money to England and America, endowing the prizes known as the Rumford Medals of the Royal Society and of the



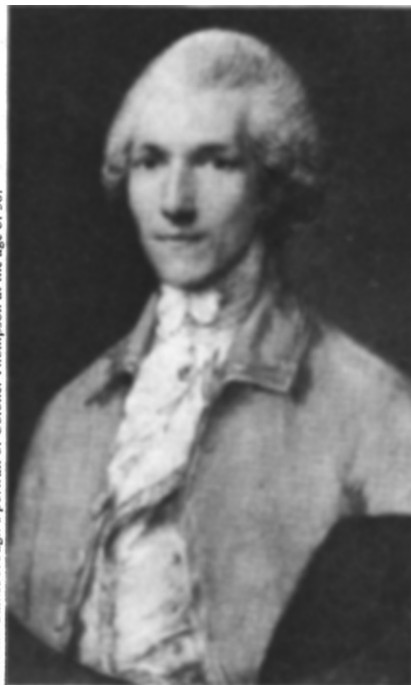
American Academy of Sciences. Instructions in his will established from the residue of his estate the "Rumford Professor of the Physical and Mathematical Sciences as applied to the useful Arts" at Harvard. These eponymous memorials furthered his imprint on the scientific community. Perhaps Rumford was not unique, for adventurers of all sorts abounded in revolutionary times. Moreover, the history of technology provides us with occasional examples of scoundrels — but Rumford must be unique in the degree of his rascality. He appears to have been a man devoid of real allegiances, clever enough to exploit the political turmoil and the scientific conceptual turbulence of his times for his own good.

Sanborn C. Brown, Emeritus Professor of Physics at MIT, presents the fruits of nearly 40 years of search for the original documents relating to Rumford's career. For these efforts every subsequent scholar will be grateful. In 1962 Brown wrote *Count Rumford Physicist Extraordinary*, one of the publications in the Science Study Series, which related in a chronological way what he had learned of Rumford's life. Brown's new biography expands upon that earlier book without deviating from its patiently explanatory tone and linear structure.

Brown, of course, has not been alone in paying attention to Rumford's life and accomplishments. Many historians of science, historians of technology and sociologists of science have examined portions of Rumford's activities, and it is to the neglect of this informing and analytical literature that Brown's biography owes its greatest weakness. Examples are not difficult to come by. Robert Fox in *The Caloric Theory of Gases* (Oxford University Press, 1971) has examined Rumford's work on the nature of heat, and placed Rumford's ideas within their contemporary milieu. Although Brown cited Fox's work, he did not use it to inform his own discussion. Morris Berman has analysed the early days of the Royal Institution in *Social Change and Scientific Organization: The Royal Institution, 1799–1844* (Cornell University Press, 1978). While Berman's general interpretation and analysis is the subject of considerable debate, much of what he has said is both useful and informative to the biography of Rumford. Brown appears to have been unaware of Berman's contributions.

Reading the biography of any practising scientist (even a part-time scientist, as Rumford assuredly was) raises the question: what is the most important aspect of such biography? To be sure, the biographer owes his subject as accurate a chronicle of the chief events in the life as can be achieved. Brown has provided that for Rumford. But the biographer of a scientist also owes his reader the benefits of his analysis of the subject's endeavours, of his achievements as seen by his

contemporaries, of his failures as judged by his contemporaries, and an assessment of where the subject fits into the histories of science and of technology. While Brown discusses Rumford's work on the nature of heat chronologically, he has not truly shown what motivated Thompson's extensive experiments. What is missing is the deep analysis which a historical study requires. When Brown discusses Thompson's social reforms he chronicles the major efforts, but he fails to show whether Thompson regarded his projects as intellectual solutions to complicated puzzles, as palliatives to grievous human needs or as a means for currying personal favour for himself. Above all, Brown does not appear to have realized how much of an outsider Count Rumford was; the narrative suggests that Rumford functioned at his best only when he enjoyed a perverse, self-engendered alienation. Perhaps, in this instance, the abiding question should have been "What made Rumford run?"



Thomas Gainsborough's portrait of Colonel Thompson at the age of 30.

Physicists publishing research findings must do more than offer raw data. Generally, that data has to be placed within a larger conceptual framework. The historian and the biographer have the same obligation. To view a man as a two-dimensional cardboard cut-out (no matter how colourful), jerked into spasmodic action by unseen wires and strings against a flat backdrop, impoverishes our understanding of him. It is on this score that Brown's biography of Rumford is most disappointing. Brown's biography is not the definitive life of Rumford, but it does offer a brave factual beginning on which subsequent analysis must rely. □

Mrs. Fullmer is Professor of the History of Science at Ohio State University, Columbus, Ohio.

Of special concern

Austen Albu

Science and Technology: A Five-Year Outlook. By the National Academy of Sciences. Pp.544. (Freeman: San Francisco and Oxford, 1980.) Hardback £8.90; paperback £5.40.

IN 1977 the United States Congress legislated for a periodical Five-Year Outlook on Science and Technology. The responsibility for this work was eventually transferred to the National Science Foundation. In 1978 the Foundation asked the National Academy of Sciences to prepare a report describing the current state of significant research areas and pointing out those areas which would be of special concern within the five year period. A review procedure by members of a number of other bodies was established. Those involved included 50 contributors, 20 editorial consultants, and 170 reviewers and additional contributors. Although the resulting report is by no means comprehensive and only intended as a preliminary study, it covers a number of fields in encyclopaedic fashion and one can only express admiring astonishment that it should have been produced by March 1979.

British Members of Parliament have long envied the resources available to their American colleagues and, for all their growing interest in science and technology during the past few years, they would never have succeeded in commissioning a report on this scale. Written in a straightforward style, not over-popularized, it offers descriptions of recent and likely future developments in such fields as geology, biology and biochemistry, physics, computers, energy, and US demography and health. Some of the subjects are politically sensitive and tend to be dealt with in a rather didactic manner; but on the whole the problems requiring research in the near future seem to have been set out fairly. Of course neither the average congressman nor this reviewer is capable of judging the scientific quality of these descriptions and proposals, and the problem of choice of priorities remains as difficult as ever. I am reminded of a remark of my son's when, in his twenties, he was facing a choice in his life on which he had consulted his liberal parents: "Just like my parents, tell me all the pros and cons and leave the decision to me".

Two criticisms can be made of this report: there are no price tags attached to any proposals for the future, without which serious consideration of alternatives by a legislative body is impossible. Perhaps equally serious is the lack of any reference to Defence R&D, the cost of which overshadows all the rest and which itself

contributes indirectly to so many other fields.

The problems now facing American scientists and engineers, though vastly greater in scale, are common in advanced industrial countries. There has been a collapse of the academic boom of the 50s and 60s. Average annual growth at constant prices of funding for academic R&D from 1953 to 1960 was 12%; from 1960 to 1964, 14%; from 1968 to 1974, zero; and from 1974 to 1978, about 4%. Despite increased numbers of graduate students, doctorates in science and engineering are declining. Total support for basic academic research declined by 8% between 1968 and 1976 and federal support by 10%.

Naturally the overwhelming number of developments reported are concerned with

practical and, in some cases, urgent problems: energy, material resources and health. Their solution is difficult enough in scientific terms but is increasingly hindered by social and political considerations. These are not only panic reactions as in the case of genetic manipulation or nuclear energy after Harrisburg but arise out of growing concern with environmental questions, leading to regulatory procedures. No doubt there is some special pleading here. Even the possibility of expanding the use of coal is hindered by the opposition of agrarian states to its mining in their territories and by fear of the atmospheric effects of its use in power stations. For this reason conservation is priority number one in the energy field. Of course vested interests are involved and indications of these can be detected in some

of the reports. Political decisions can also distort the allocation of R&D resources, tactfully referred to in an account of the examination by "a new leadership" of the "enormous effort" of Nixon's "War on Cancer" in order to achieve a more balanced programme.

Many readers of this fascinating book must be left with two impressions: the enormous area remaining for scientific R&D to deal with the world's material problems and search for knowledge; and, in a divided world, the considerable international cooperation here reported and the need and scope for its further enlargement. □

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Bringing Man poetically to life

Yves Coppens

The People of the Lake: Man, His Origins, Nature and Future. By Richard E. Leakey and Roger Lewin. Pp.223. (Collins: London; Doubleday: New York, 1979.) £6.50.

People of the Lake is a kind of essay on Man. Pleasantly readable, it puts forward in a little over 200 pages an overall view of our species in both a historical and comparative context which is thoroughly satisfying for anyone with a naturalist's turn of mind. Although the reader may regret a certain lack of rigour in the comparative approach, as too great an emphasis is placed on seeking out good examples to back up the argument at the expense of a critical attitude towards similarities and differences, the attempt is nevertheless an extremely interesting one, and one worthy of being encouraged.

Divided into 11 chapters, the book is in fact made up of two sections of almost equal length; the first of these, comprising the first five chapters and part of the eleventh, is devoted to palaeontology, the origin and evolution of Man, from the first primates of 70 Myr ago to *Homo sapiens sapiens*; the second (Chapters 6, 7, 8, 9, 10 and the remainder of Chapter 11) concerns itself with ethology, human behaviour and the search within the animal kingdom for Man's origins and the direction of his evolution. Given the training and special interests of the authors, this second part is more an essay in prehistoric ethology: just like chimpanzees, gorillas, baboons or geladas, contemporary Man is used in an attempt to reconstruct the lifestyle, psychology and behaviour of ancient Man. In spite of what

might be inferred from the sub-title, neither the future of mankind, nor even his present condition, seem to be the writers' principal concern.

The point of departure — hence the title — is the palaeontological research carried out by Richard Leakey over the past 12 years on the shores of Lake Turkana in Kenya. In 1967, Richard Leakey, together with Francis Clark Howell and myself — and initially Louis Leakey, his father, and Camille Arambourg — played a part in setting up the important international expedition along the Omo River in Ethiopia, to the north of Lake Turkana itself; and it was during one of the many flights that had to be made back and forth between Nairobi and his camp on the Omo that he noticed the sedimentary formations outcropping on the eastern banks of the lake. He decided there and then to investigate, and since 1968 a dozen or so separate expeditions have provided an extraordinary collection of human fossils, their age ranging from more than 4 Myr to less than 1 Myr and including half a dozen skulls in an excellent state of preservation, highlighted by the famous ER-KNM 406 (*Australopithecus boisei*), 1470 (*Homo habilis*) and 3733 (*Homo erectus*). These expeditions, known as the Koobi Fora Project, make up one of the four large-scale international projects under way in the Rift Valley of East Africa and concerned with the problems surrounding the origin of Man. The other three are the Olduvai expedition (more recently Olduvai-Laetoli), originating with Louis Leakey as long ago as 1931 and under the direction initially of Louis Leakey, then of Louis and Mary Leakey and, since Louis' death in 1972, of Mary Leakey alone; the international Omo expedition for which Francis Clark Howell and myself were responsible over eight further seasons after the departure of Richard Leakey in 1968; and the Afar international expedition, which began work in 1972 and has since been under the supervision of Maurice

Taieb, Donald Johanson and myself. The People of the Lake themselves, of course, are these fossil men of about 2 Myr ago (*Australopithecus boisei* and *Homo habilis*), whom the authors bring poetically to life again as early as the first chapter of the book, in a reconstructed landscape on the banks of a palaeolithic Lake Turkana.

The palaeontological résumé provided is rather conventional but not without interest. It traces the links between Cretaceous and Quaternary forms, the change from a nocturnal to diurnal life (and the consequent improvement in eyesight), and, much nearer in time to ourselves, the straightening up of the body and its probable successive implications — the freeing of the hands, work, development of the sweat glands, loss of hair and the appearance of a protective pigmentation for the body. Considerable passages of the book are devoted to this possible ancestor of Man, the quadruped *Ramapithecus*, and his erect descendant the *Australopithecus*, and to the irritating gap of 4 Myr that separates them.

The ethological aspects examined in the second section of the book represent in many cases interesting approaches to problems which are not usually dealt with in the context of prehistory. They all arrive at the same fundamental conclusion: that Man is as he is because at the time of his origin he shared his food with the other people in his group — the development of his intelligence (and of his language, which is no more than a manifestation of his intelligence), the extraordinary transformation of his technology, his inclination for reciprocal altruism and for family life, even his obsession with sex — are the fruits of his cooperative life and of his mixed economy based on the male's hunting and the female's gathering: everyone is in part dependent on others. With this idea established as their foundation, the authors go on to debate a wide range of problems raised, rightly or wrongly, by our times, or by other writers whose success has

moulded our view of our times: the hypothesis of the essential role of hunting in the evolution of Man, the counter-hypothesis of the preponderant role of gathering, the problem of Women's Liberation, the still unsettled question of the origin of aggression and of the phenomenon of war, and so on.

It is unfortunate that these topics are touched on only rather superficially, though it could scarcely be otherwise in such a limited space; from this point of view it must be admitted that the reader's appetite is never entirely satisfied: he is left not wholly convinced by an argument too fragmentary in its evidence. Nonetheless, the very fact that some additional light has been thrown on all these various problems by authors possessing zoological, ethological, prehistorical, palaeontological and ethnological knowledge, obviously deserves a salute. Indeed, a second salute, as I have already stressed the importance of a comparative approach in the introduction to the French edition of the present authors' earlier work *Origins* (for a review, see *Nature* 270, 108; 1977).

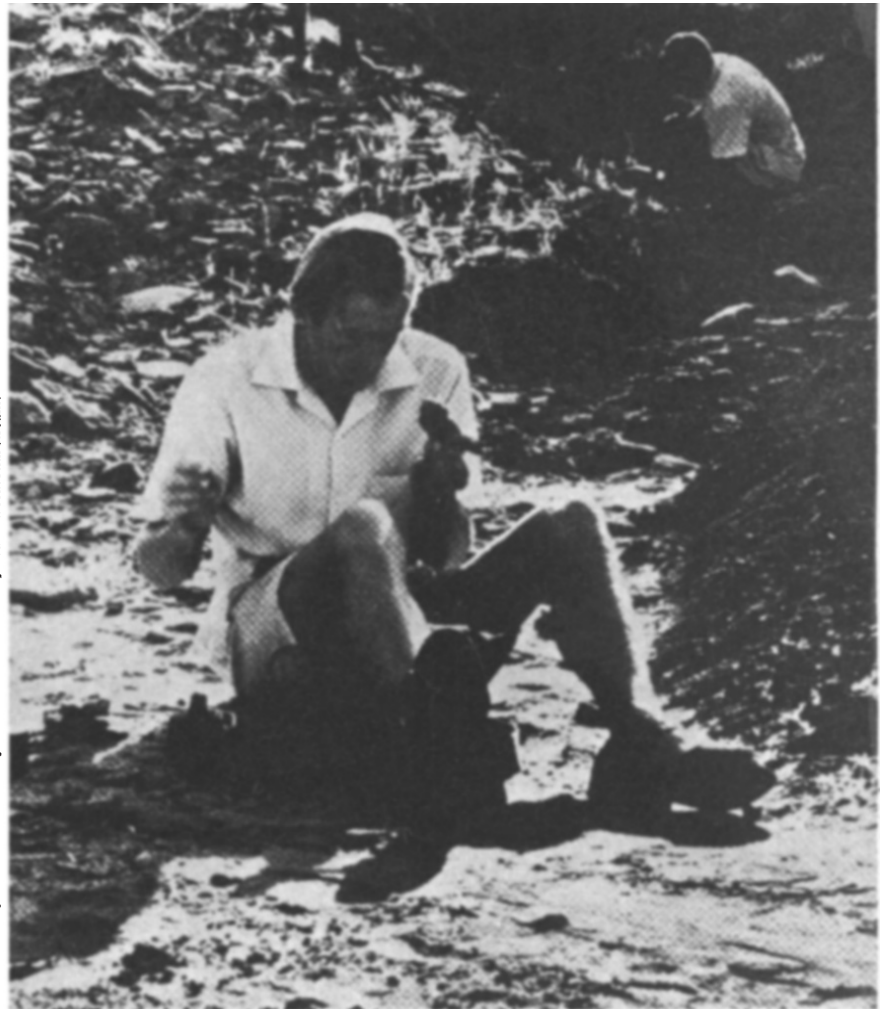
Nevertheless, I cannot agree with every statement made in this book. It may be instructive to look at some of the points of disagreement. On page 73, the *Australopithecus* is introduced to us as a possible herbivore although there is little likelihood that it can actually have been one; he was in fact not sufficiently far removed from Man to have had a fundamentally different digestive apparatus. And Man is incapable of digesting cellulose.

Between 1955 and 1960, Johannes Hürzeler, in connection with his study of the *Oreopithecus*, pointed out the great antiquity of the phenomenon of bipedality in nature. A demonstration (page 70) of the existence of bipedality only 4 Myr ago is not therefore going to come as much of a surprise to vertebrate palaeontologists, who for the past twenty years have been expecting to uncover its existence in the Miocene.

The notion (page 131) that the brain was among the first organs to take on human form, rather than the contrary suggestion, has long prevailed; hence, the celebrated Piltdown hoax of a fossil man with a modern brain and pongid teeth. And when authentic and extremely ancient fossils, such as the first Taung *Australopithecus* skull, have been brought to light, it has been quickly established that the brain, although of modest development, was different from that of the great apes.

One should not confuse (page 92) written knowledge with the knowledge of nature that can be acquired in the field; any farm worker in any Western country, living in close contact with his surroundings and often hunting game there, would surprise Richard Leakey and Roger Lewin with the quality and diversity of his knowledge of the fauna and flora, even of the very rocks, of the countryside about him.

How is it possible (page 126), in a work



Richard Leakey examines a hominid jaw while seated on the dry bed of a seasonal stream.

which (rightly) attempts to demonstrate the precarious nature of the borderline between the apes and Man, in respect of the use (and manufacture) of tools and the notion of natural facing elements (the rain dance of the chimpanzees, for instance), to come out with a statement as specific as "we suggest that it was primitive *Homo* . . . who made tools and, furthermore, that it was only *Homo* who did so"? Why is it not possible that the *Australopithecines* may have made a beginning which Man inherited and then developed brilliantly? Linking the tool so dogmatically to the genus *Homo*, the authors throw the definition of the genus into confusion — a definition which should remain strictly biological.

May I finally be permitted to give vent to my irritation at the habit, not at all international in spirit, of expressing all measurements in inches, feet, yards, miles and square miles; in 1980 we are all citizens of the world!

These differences are, it need hardly be said, extremely minor in the context of an overall impression of a highly intelligent work, which has the great merit of linking natural sciences and the humanities.

Let me now turn from these basic issues to make a few remarks on form. As I pointed out at the beginning of this analysis, this is certainly an admirably

readable book. It is made so by its style, and by an approach which, though scientific, is always deductive. It is enlivened by the use of entertaining asides, such as anecdotes like those describing how Tom Gray and Donald Johanson discovered Lucy, the context in which Ralph Holloway studied his endocranial casts, or the anxious cleaning of skull 3733 by Alan Walker; and, not least, by the interrogative way which they sometimes use to outline the problems better; the questions asked are precisely those the reader is likely to be asking himself.

We learn by this book that Man is very ancient; his origins, both morphological and behavioural, have deep roots in the animal kingdom. It is by studying this kingdom and systematically analysing all contemporary human societies that we may come up with the answers to questions about our own origins, the process of our evolution and, consequently, to some limited extent, about our future. It is only through a better knowledge of our past and of all that can illuminate it that we may come to be able to foresee, and forestall, our destiny. □

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Oceanographic laboratories

A. S. Laughton

Oceanographic Institutions — Science Studies the Sea. By P. Limburg. Pp.265. (Elsevier/Nelson Books: New York, 1979.) \$9.95.

THE public image of a scientific laboratory and of the scientists who work in one is highly coloured by what is seen on television and by science fiction. It is therefore very valuable for a book to be published which explores the reality of research institutes and attempts to look at the motivation and day-to-day working of research scientists. By choosing a subject such as oceanography which has grown rapidly during the past three decades, but the roots of which go back to the last century, it is possible to relate the development of the science with the development of the laboratories studying it.

Limburg has written a book that tackles with varying success both the science and

the laboratories. As a potted history of oceanography his first three chapters leave a good deal to be desired. There are inaccuracies (the first word in the acronym ASDIC is "Allied" not "Anti-"), a lack of understanding of some of the technology (the difference between reflection and refraction seismics) and a rather journalistic view of the development of scientific thought (from continental drift through stable continents towards plate tectonics). The historical review is very unbalanced. Two pages are devoted to diving and subsea habitats, whereas deep-sea drilling is limited to 11 lines, and oceanography from space to six. Near-bottom technology scarcely gets a mention nor do the achievements of long-term deployments of instruments in the ocean, which depend crucially on the developments of marine acoustics.

The accounts given of the work in the major (and some minor) oceanographic laboratories in the USA are both informative and interesting, and the unravelling of the complexities of US Government agencies' funding of ocean research is revealing, if perhaps somewhat

ephemeral. As the book proceeds, it becomes clearer and clearer that the author is really concerned with oceanography in the USA and I wonder why this was not made clearer in the title. Oceanography in all other countries is limited to one chapter and appears to include only those laboratories which the author was able to visit. No mention at all is made of oceanographic institutions in the USSR or in Japan. It cannot therefore be said, nor is it claimed in the Introduction, that the book is a comprehensive review of oceanographic institutions. What it does provide, and in this is its success, is a peep in through the doors of some of the laboratories, an insight into the enthusiasm and motivation of some oceanographers both great and small, and an indication of the breadth of the subjects studied.

For a student wishing to start a career in oceanography, especially in the USA, I warmly recommend this book. □

A. S. Laughton, Director of the Institute of Oceanographic Sciences of NERC, has spent his working life in the field of marine geophysics, and has worked closely with several of the major oceanographic institutions in the USA.

The wonder of it all

R.H. Dalitz

Knowledge and Wonder: The Natural World as Man Knows It. Second edition. By Victor F. Weisskopf. Pp.312. (MIT Press: Cambridge, Massachusetts, and London, UK, 1980.) Hardback \$15, £9.30; paperback \$5.90, £3.70.

TODAY we have knowledge of our Universe covering distances from 10^{-16} cm to 10^{+28} cm, and for time intervals from 10^{-24} s to 10^{+17} s. All the phenomena observed within these limits are apparently governed by a very limited number of physical forces. The aim of this book is to lead non-scientists to appreciate the wonder of this situation, first by indicating simply how we know the facts and then by illustrating the wide variety of phenomena governed by these forces. Such an endeavour necessarily involves some oversimplifications, of course. Here the job is done in low key, showing how simple concepts based on the laws governing these forces can lead to an understanding of this strikingly wide variety of phenomena.

The book begins with space and time. It estimates the lifetimes of Earth and of the Universe, and the sizes and distances in the visible Universe. Two major forces are obvious to us from daily life, gravitation and electromagnetism. Their characteristics are outlined and related to the variety of phenomena to which they give rise. We

explore the Solar System, understand the Milky Way as our galaxy, and relate it with far-off galaxies, made visible by telescopes. On Earth, we explore the structure of the atom and learn of its tiny nucleus, first deduced from Rutherford's alpha-particle scattering experiments. Both electrons and light show both particle and wave characteristics, and we soon reach quantum mechanics, with discrete energy levels for atoms, each level corresponding to a standing wave of electrons circulating about its atomic nucleus. The Coulomb force accounts for the binding of these circulating electrons to the nucleus, and also for the forces between atoms. We are shown the most common bonding mechanisms between atoms, and use the atoms as building blocks to make molecules, crystals and other solids and liquids, and so account for the materials around us as due to electromagnetic forces.

Next we are led to still shorter distances, to the nuclear and subnuclear worlds. The atomic nucleus consists of protons and neutrons, bound together by strong nuclear forces, and we soon understand why the stable nuclei are limited in number. An atom's chemistry then depends primarily on the proton number of its nucleus, and a pure chemical element can have various atomic mass values, corresponding to the number of neutrons in its nucleus. The ancient dream of transmuting other elements into gold is now seen to be possible, although so expensive that it has no financial value.

Recent experiments on the scattering of electrons and neutrinos by nuclei have shown that the nucleons themselves are

composite, each containing three quarks. The rich spectroscopies of atoms and nuclei, each due to their multiplicity of component particles, are now paralleled by a spectroscopy of nucleons, due to excitation of the motions and spins of their component quarks. These experiments also show us that quarks, as well as electrons and neutrinos, have no detectable structure larger than 10^{-16} cm.

We turn now to our terrestrial environment. Earth's long history is traced out, great changes occurring in its surface structure long before Man's emergence in recognizable form through evolutionary development. Thus, we come to the story of Life itself. The great steps are traced out, although not in detail, for we do not know them. We understand how the basic molecules needed to make up living material could form, perhaps through lightning flashes on the Earth's surface. The energy falling on Earth from the Sun was essential for the subsequent development of more complex organisms. Earth became covered in green, due to the chlorophyll which uses the energy reaching us from the Sun so efficiently. In the course of time, more complex organisms evolved, their structure being determined by the double-stranded DNA molecule, a thread carrying instructions for the construction of a living being, a thread replicated in its every cell. This evolution from simple to complex organisms, to living beings and to *Homo sapiens*, cannot be traced in detail but the general principles are outlined. The important role of mutations in this development is emphasized, and we see how the life on earth today could plausibly have

resulted from such an evolutionary history. Finally, we recognize that evolution in Man's behaviour has become rapid relative to his lifetime, since mankind now gains new knowledge quickly and disseminates it efficiently, so that new situations for humanity can now develop in the space of years. This evolution is of a new kind, not concerned with Man's structure but with his ability to control and change his environment. It is still quite unclear where this new evolution may lead us.

This story is recounted in simple terms, with compelling logic. The original lectures were for a high-school audience, and this extended version is still suitable for them. But it has much interest for all, and I hope that it will become more widely known. It could perhaps be simplified more, or extended in several places where questions are left unanswered. The book will be valuable for scientists, since few are familiar with all of the areas covered. The presentation is stronger and more tightly argued in the earlier chapters concerned with chemistry and physics than in the chapters concerning Life, where the author's knowledge comes from other scientists. The story of Life is much more complicated than that of inorganic matter, and can only be outlined in such a short space.

Each reader will have his own short-list of significant topics not covered in this book. For example, the uniqueness of our situation on Earth might have been underlined by a brief comparison of Venus with Earth; why have two planets of such similar constitution, mass, and so on, turned out to be so different today. Again, neutron stars are well known now, some of them as pulsars, of which there are many in the sky, but they are not mentioned; nor are quasars nor black holes, but these are not yet understood and they lie on the edge of Einstein's gravitational theory, which the author consciously chose to omit. At the other extreme, the book gives no hint of the complexity of the quark world; we know 15 kinds of quark today, and other fundamental entities are also needed. There is no mention of the weak-decay interactions, the unification of which with electromagnetism to form 'the electroweak interaction' has been such a major theoretical triumph recently. Such topics would perhaps complicate the story without adding much enlightenment, but still they are part of our everyday world. Even the strange particles are being formed around us by the cosmic radiation incident on Earth, although they quickly decay, and this cosmic radiation can be made visible with simple equipment which can be constructed in any home; it is part of our environment. Perhaps the book should have a few appendices, which could take the interested reader further towards the extreme boundaries of present research. I found few errors in the book, but it is stated on p.179 that the highest energy accelerator today, producing proton beams up to 500

GeV, is in operation at the Brookhaven National Laboratory, Long Island; it is actually at the Fermi Laboratory, near Chicago. It also puzzled me that more than half a page should be devoted to the outmoded 'continuous creation' theory of the expanding Universe, definitely excluded by observation for some time past.

In conclusion, I can only say that this book is superb and far ahead of any other global accounting of our Universe, in both the large and the small. We all have friends and relatives who question us often about

modern science; this is the book to place in their hands. It shows the physicist at his best, accounting for what he sees by simple order-of-magnitude estimates. The story of Life is less amenable to this approach, and so those chapters are more descriptive. However, as it now is, the book already satisfies a great contemporary need. □

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Star maps, old and new

David W. Hughes

The Sky Explored: Celestial Cartography, 1500-1800. By D. J. Warner. Pp.293. (Alan R. Liss: New York; Theatrum Orbis Terrarum: Amsterdam, 1979.) \$70. *True Visual Magnitude Photographic Star Atlas.* By C. Papadopoulos. (Pergamon: Oxford and New York.) Three volumes. Vol.1. *Southern Stars*, 1979; £100, \$225. Vol.2. *Equatorial Stars*, 1979; £145, \$325. Vol.3 *Northern Stars* (by C. Papadopoulos and C. Scovill) publication due June 1980.

SINCE the dawn of history, mankind has been a fascinated observer of the heavens. The stars were arranged into groups of constellations and names were given to these constellations and also to many of the brighter stars. However, without a good celestial map the night sky simply dissolved into a trackless confusion of points of light. Maps were also a simple means of recording the knowledge gleaned from celestial observations. In the old days the constellations were depicted by artistic, symbolic figures, and many of the early star maps and globes became much more than just scientific aids but were also beautiful works of art in their own right.

The first important extant star catalogue was compiled by Ptolemy in AD 150 and contained 1025 stars grouped into 48 constellations. Ptolemy's work was the cornerstone of mediaeval star catalogues and, for more than 1,400 years, Islamic and European astronomers limited their observations to the stars listed by Ptolemy. The catalogue of Al-Sufi in the tenth century, Ulugh Beg's Alfonsine Tables in the fifteenth century and the Copernicus catalogue in the early sixteenth century were simply revisions of Ptolemy. These catalogues were usually just lists of stellar latitudes, longitudes and magnitudes.

Most astronomers found that a graphic form was more useful than a catalogue, and so maps of the sky were introduced. The first flat star map of any significance was published by Albrecht Dürer, the great Nuremberg artist and mathematician, in

1515. The date reflects both the development of printing and the upsurge in the study of astronomy initiated by such worthies as Copernicus, Regiomontanus and Piccolomini. The era of great celestial cartography probably ends with the publication of John Elert Bode's *Uranographia* in 1801. Bode used 99 constellations and included 17,240 stars down to eighth magnitude. After Bode the constellation boundaries became more rational, the constellation figures slowly disappeared and star maps lost much of their charm and beauty becoming the dull procession of differing sized dots we know today.

Deborah J. Warner, the curator of the History of Astronomy at the National Museum of History and Technology, Smithsonian Institution, has produced a comprehensive catalogue of all the flat star maps printed in Europe between the fifteenth and early nineteenth centuries. Also included are details of a few globes for comparison. It is a work of considerable scholarship and, as a source of detailed information, will be a much valued reference book for decades to come. The listing is alphabetical and the work of each cartographer is analysed according to the number and size of maps produced, the limiting star magnitude, the co-ordinate system adopted, the date and place of publication, and the non-Ptolemaic constellations introduced. It is intriguing to note how certain constellations come and go. For example Lalande charted Felis, his own cat, and also introduced his telescope as Quadrans Muralis. Thomas Young squeezed the Battery of Volta into the space between the head and hooves of Pegasus. Julius Schiller even attempted to de-paganize the complete zodiac by introducing the twelve apostles in the place of Pisces, Aries, Taurus, Gemini and so on. The Warner catalogue is profusely illustrated in black and white and the beauty of these illustrations is most impressive. Obviously only a few of the maps were coloured and even this would probably have been a laborious task of hand water-colouring. It is hard to fault this book but may I make a suggestion for a new venture. If Deborah Warner produced a large-format book, with some colour prints depicting the best artistic

representations of each constellation, I'd be first in the queue with my money.

The transition between star maps, the beautiful, and star maps, the functional tool of modern science, is illustrated perfectly by the second book under review. Papadopoulos has produced a large set of maps by photographing the sky through a four-element astrographic objective lens manufactured by Carl Zeiss (Oberkochen). This lens had a diameter of 12.5 cm, a focal length of 62.5 cm and gave a negative with a usable field of 15°. Each map has been produced from the original negative by line reproduction, a system which prints only black, and has no halftones. This is ideal for stars but gives a very poor reproduction of the appearance of galaxies and nebulae.

The *Southern Stars* volume contains 120 maps covering the sky region from declination -30° to the South Celestial Pole. Each map covers an area of 11° by 11° and there is a minimum overlap of at least 1° between neighbours. The *Equatorial Stars* volume contains 216 maps and covers the region between -30° and +30° in declination. A set of transparent co-ordinate overlays are provided, six for Vol. 1 and three for Vol. 2. The system is converted to 1950.0. Each map measures about 33 cm by 33 cm. Volume 3, *Northern Stars*, will be published later this year.

The atlas is designed to show all stars which are brighter than visual magnitude 13.5 and, to quote the author, "the final print produces star images with easily readable differences in their diameters according to their magnitude". The accuracy with which the magnitude can be assessed obviously depends on the accuracy with which the star image diameters have been produced on the map and on the accuracy of measuring these diameters (a star of third magnitude is about 1 mm across). Superficially each map appears like a white sheet speckled with tiny black dots, surrounded by a black border with right ascension and declination markings on them so that the transparent grid can be positioned. To find your way between maps you are given a rather inadequate, small-scale, chart index. You actually need at least a copy of Antonín Bečvář's *Atlas Coeli* and preferably a set of Smithsonian Astrophysical Observatory star charts as well. The main advantage of the Papadopoulos atlas is that it goes down to visual magnitudes 13.5 as opposed to the SAO which stops about ninth. The main disadvantage is the price, but assuming you need to go down to 13.5 mag (and I would be very surprised if the number of astronomers who do really need to do this is more than minuscule) £245 for a collection of 336 Papadopoulos maps — Vols. 1 and 2 — is what you have to pay here. I must confess that I won't, and neither will my university. □

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Thriving young discipline

Martin Daly

Human Sociobiology: A Holistic Approach. By D.G. Freedman. Pp.192. (Free Press/Collier Macmillan: New York and London, 1979.) £8.50. *The Genesis Factor.* By R.A. Wallace. Pp.256. (William Morrow: New York, 1979.) \$9.95.

HUMAN sociobiology seems to be a thriving young discipline, if a spate of books and new journals over the past two years is to be taken as evidence. Perhaps much of this activity will eventually be seen to be the cloaking of old ideas in new jargon, but much of it is truly synthetic. Sociobiology is gaining adherents among social scientists because, rather than contesting their established theoretical approaches, it is for the most part metatheoretical to their usual levels of analysis. Theories of sex role socialization, for example, may accurately describe processes by which masculine and feminine behaviour develop, while offering no insight into the cross-cultural consistency of the outcomes. Or a potlatch may be explained as a status competition, while taking it as a given that status is a commodity for which men will find it reasonable to compete. Sociobiological theory attempts to deal with some of the questions that structural, descriptive theories of human action fail to address.

If a "new synthesis" can indeed be said to be emerging, it is not surprising that anthropology, the social science most informed by evolutionary thought, is leading the way. However, sociobiological schools of thought have also surfaced in economics, sociology and even political science. The laggards whose participation is most seriously missed are psychologists.

The grounds for thinking psychology a crucial missing element in a productive synthesis are as follows. Anthropologists and other scientists testing sociobiological hypotheses have been concerned to demonstrate measurable fitness consequences of extant variation in behaviour, and they have achieved some notable successes (see, for example, chapters in *Evolutionary Biology and Human Social Behavior: An Anthropological Perspective*, edited by N.A. Chagnon and W. Irons; Duxbury Press, 1979). However, a good deal of modern human behaviour is manifestly not fitness-maximizing. Must sociobiology therefore be mute about its origins? Not if appropriate models of the structure of human motives and cognitive processes can be developed. The psychological mechanisms proposed must be anchored simultaneously in sociobiological accounts of their phylogeny and adaptive significance and in predictions of their behavioural outputs, including predictive

statements about the kinds of circumstances in which such evolved mechanisms can be expected *not* to produce adaptive outcomes. This task offers experimental psychologists a gold mine of opportunity for research and for theory development.

Daniel Freedman's *Human Sociobiology: A Holistic Approach* constitutes an early psychological foray into this rich field, and while the effort is deeply flawed, it nevertheless unearths several nuggets. The book's greatest shortcoming resides in the fact that Freedman ventures out of his depth in population biology. This produces two extremely distracting muddles. One, which surfaces repeatedly, concerns the significance of populational levels of heterozygosity. Freedman imagines that inbreeding is selected against not because of any deleterious effects of homozygosity upon the offspring of consanguineous unions, but because it constitutes "bad evolutionary planning" by reducing populational reservoirs of heterozygosity. He proceeds from this basic misunderstanding to characterize human populations as more or less heterozygous according to their marriage practices, a train of speculation which in Chapter 7 leads to several odd conclusions which leave the author needlessly vulnerable to accusations of racism.

The second major muddle is conceptually related to the first. Freedman has borrowed his University of Chicago colleague Michael Wade's suspicion of

MESOZOIC MAMMALS

The First Two-Thirds of Mammalian History

Edited by Jason A. Lillegraven

Mammals have been an important part of the earth's land fauna for at least 200 million years. This book offers a comprehensive review of what is known about mammalian life during the first two-thirds of its history — the Mesozoic era — before the extinction of the great reptiles. The book provides biologists and geologists with encyclopedic listings of fossil locations, recognized taxa, and temporal-geographical occurrences of genera along with descriptions of the anatomical features of each group and extensive bibliographies. Interwoven with the summaries of data are valuable interpretive — sometimes admittedly speculative — discussions of the origins of mammals, their patterns of distribution, and their biological, behavioral, and evolutionary relationships during the "dark ages" of mammalian history. Illus. 320 pages £21.00, cloth £5.75 paper.

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"selfish gene" reductionism without apparently grasping the rationale for that suspicion. The undisputed fact that social groups have complex interactive properties which may be mistaken by researchers focussing attention on individuals is thoroughly confused with the question of the roles of group versus individual selection in the evolution of adaptation. I am not cavilling on this subject, for the author's imagined critique of individual selection is a major, recurring theme of the book. He begins Chapter 4 with this paragraph (page 45):

I hope I have by now established that both the selfish-gene approach from below and the group selection approach from above are needed for an adequate explanation of social behavior, no matter the species. When we get to primates, the need for a dual causality model rather than a one-way model is emphasized since primates can act with clear selfishness or in concert, as the situation demands.

By this point, sociobiologically sophisticated readers may have decided to take a walk, and that is a pity, for the book's strengths lie just ahead.

One strong feature is Freedman's cross-cultural review of sex differences in the behaviour of human children. Cultural anthropologists have seldom standardized methodology or collected quantitative data, so the psychologist's touch is valuable here. Among the many interesting phenomena described are the prevalence of acknowledged hierarchies of "toughness" among small boys and the irrelevance of the same dimension for girls. Also intriguing are data on children's drawings, in which thematic sex differences are highly consistent across cultures even as substantial between-cultures variation is manifest.

Fifty student research projects carried out under Freedman's aegis (and outlined in a fifty-page appendix) are discussed in the text. Though highly variable in quality, these studies introduce a number of innovative methods and provocative findings. For example, modified TAT cards (pictures about which subjects are invited to tell a story) are used to assess the emotional impact and status significance of facial hair and of relative height, with results that sometimes contradict the respondents' professed attitudes. In a similar vein, the dimension of kinship is added to another standardized psychological instrument, Kohlberg's Moral Dilemmas, in order to provide evidence that people apply different moral codes to kin and non-kin. The use of such techniques should inspire readers interested in developing a sociobiological psychology.

The concluding chapter, entitled "Biology or culture?" begins with a not altogether successful attempt to dismiss such unproductive either-or-ism: "There is no way to separate the genetic and the learned for they are permanently glued together, and that is true everywhere in nature, including man". Heaven knows what is meant by "learning" if it is

manifest "everywhere in nature", but most readers will probably find the sentiment acceptable if not the logic offered in support of it. Freedman then reviews his own convincing evidence for population differences in human infantile behaviour and temperament, proceeds to speculate plausibly that such differences may be causal to non-arbitrary cultural differences, and somehow concludes that racial "antagonisms are more or less built into the species" and that "the only sound solution lies in miscegenation". One is left perplexedly leafing back, vainly trying to reconstruct the extraordinary logic of this argument.

Robert Wallace's *The Genesis Factor* is blurbbed as "the first popularly written book on sociobiology". If popular writing consists in talking down to one's readers and in disdaining to cite sources, the characterization is correct. Popular writing also apparently entails a suspension of respect for fact. Here are just three examples: parental care occurs in "only a few isolated species" of fishes (page 37); gorillas are "highly promiscuous" (page 76); and the administration of androgens to infant female mice transforms them into killers

without interfering with normal oestrus (page 116). Even where a particular experiment is under discussion, Wallace is liable to serious mis-statement of the results, as in his treatment of Harry Power's mountain bluebird removal experiments (page 179). Running roughshod over such disparate subjects as IQ and the alleged adaptive value of death, Wallace's book will regrettably reinforce antibiological prejudices of some social scientists while proving an embarrassment to thoughtful biologists. It ends on an even more remarkable note than Freedman's book, namely the author's personal foreign policy recommendations for the United States government! With friends like Wallace, sociobiology has no need of enemies.

Let us hope that we will see more and better empirical work of the sort initiated by Freedman. And may we have fewer grand statements of the policy implications of human sociobiology, at least until the discipline grows up a little. □

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Orcadian heritage

Euan W. MacKie

Investigations in Orkney. By Colin Renfrew. Pp.234. (Society of Antiquaries: London. Distributed by Thames and Hudson: London, 1979.) £20.

UNTIL the recent oil discoveries the Orkney Islands seemed remote from and peripheral to modern Britain, yet several times in the past they have been at the crossroads of important movements of people and influences. These usually came either up the western seaways from Atlantic Europe, like the stone tomb-builders of Neolithic times and elements among the inventors of the Iron Age tower-forts known as brochs, or, like the Norsemen and the Early Bronze Age Beaker people respectively, westwards from Scandinavia or north-westwards from the Low Countries or the eastern mainland of Britain. Like the churning cross-currents of the Pentland Firth, these genetic and cultural mixtures often produced periods of striking local developments in the islands. The sandstone of which these are formed provided ideal building material and some remarkable developments in drystone architecture resulted.

Archaeological research in Orkney has been sporadic in the past with long periods of inactivity, but the early 1970s saw a burst of new work by visiting excavators. Sites

like the Neolithic dwellings at Skara Brae and Knap of Howar, the Pictish and Norse village at Buckquoy, the prehistoric and early historic settlement at Skail and the Neolithic chambered tomb at Quanterness (the main subject of the book) were explored with modern techniques for the first time. Knowledge is still advancing rapidly through the efforts of a new resident team of archaeologists financed by the Scottish Development Department, the North of Scotland Archaeological Services Unit based at Finstown.

Professor Renfrew has long been concerned to explain in modern terms the dramatic changes visible in the prehistoric archaeological record, such as the appearance of the chambered burial mounds of Atlantic Europe at around 4200 BC, and he favours hypotheses of indigenous development rather than any involving the diffusion in some way of ideas and skills from more advanced cultures. The radiocarbon dates for the earliest European chambered mounds have in any case shown them to be older than those of the Early Bronze Age in the Mediterranean world which were originally suggested as prototypes. Another aspect of this new approach is the interpretation of the tombs in terms of the kind of societies which built them and of the environment in which these people existed. The cairn at Quanterness is the first in Orkney to have been investigated with these general problems in mind.

One example of the detailed mass of evidence collected may be cited.

The Knowe of Yarso, Rousay.



Quanterness is an architecturally sophisticated tomb and its period of use was determined by radiocarbon measurements to be between about 3200 and 2500 BC. (This itself is important as it shows clearly that the tomb is relatively late in the Neolithic period and in the Orkney series, refuting again the old diffusionist theories which demanded that the most

sophisticated buildings were built first, by the original skilled colonists.) The number of individuals buried collectively — generation after generation over a period of 550 ± 180 years — in this great roofed stone vault was estimated from the thousands of disarticulated human bones recovered to be about 400. Both sexes and most age groups were represented, and the

conclusion drawn is that all the members of a small farming community of 13–20 people were buried in the cairn.

These results, and those of the many other detailed researches carried out, form a landmark in our understanding of Orcadian Neolithic society — a mass of hard evidence which will provide a minimum standard for all future work. My only doubts concern some of the conclusions drawn from the data, particularly the failure to consider in any way — even simply to dismiss it — the alternative social interpretation of these cairns. This, argued in some detail in my book *The Megalith Builders* (reviewed in *Nature* 270, 644; 1977), suggests that the people buried in the cairns could have been the members of a non-farming section of the Neolithic population, most probably of religious orders which were linked genetically and by shared architectural and funerary traditions to the other cairn builders all over Atlantic Europe. The work at Quanterness has not disproved this interpretation unless this is considered to have been achieved by the author firmly looking the other way. □

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Immortalized in a virus

D.H. Watson

The Epstein-Barr Virus. Edited by M.A. Epstein and B.G. Achong. Pp.459. (Springer: Berlin, Heidelberg and New York, 1979.) £34.50.

EPSTEIN-BARR (EB) virus was discovered in 1964 by the two workers immortalized in its name; they identified particles with the characteristic morphology of the herpes-virus family in lymphoblasts cultured from a lymphoma of a type first described in African children by Burkitt in 1958. The first chapter in this volume is an account by Epstein and Achong of this early work in which Epstein's inspiration was to examine cells cultured from the tumour after an earlier fruitless search in biopsy cells.

Since 1964 a formidable body of published work has been amassed by workers from a wide variety of disciplines using an impressive array of biochemical and biological techniques. The remaining articles in this volume, written by a number of those who have made notable contributions, summarize and review progress since 1964.

Serological studies played an important initial role, and an early chapter by Ernberg

and Klein is appropriately devoted to an account of EB antigens. This sets the stage for a description by the Henles of their notable serological studies, which not only identified EB virus as distinct from other herpesviruses but most unexpectedly showed that the virus was ubiquitous, often occurring in leukocytes of healthy donors in many parts of the world. This led to the link between the virus and infectious mononucleosis and also with a second rare malignancy, nasopharyngeal carcinoma. Plainly the major stumbling block was the lack of a unique or even consistent relationship of the virus to the malignant conditions.

Molecular hybridization studies, excellently described in articles by Pagano and Shaw and by zur Hausen showed, however, that virtually all nasopharyngeal carcinomas and African (if not American) Burkitt lymphomas contained virus DNA.

There still remained the difficulty of explaining why such a widely distributed virus apparently produced tumours in restricted geographical areas. Burkitt himself suggested that a malarial co-factor might account for the lymphoma but although, as indicated in a later chapter by Epstein, there is supporting evidence for this idea, it would seem that it cannot be a unique co-factor given the comparative rarity of the lymphoma, even in its favoured locale, in doubly infected individuals. Others have argued for strain

variations in the virus and this is discussed in the chapter by Pagano and Shaw as well as by Miller.

A further twist to the tale is provided by the recognition that the virus DNA may sometimes exist in the cell in a non-integrated ('episomal') form and at other times be integrated as described here by Adams. He, like Pagano and Shaw, envisages the rare change to malignant transformation from non-malignant proliferation as being mediated by insertion and operation of transforming sequences.

The other chapters are equally absorbing, dealing with aspects as varied as the recent IARC prospective sero-epidemiological survey (de Thé; see also *Nature* 274, 756; 1978), prospects for vaccine control (Epstein), and comparative studies on oncogenic herpesviruses (Deinhardt) — to make what must in the space available be an arbitrary selection.

The volume will be useful to those from many disciplines in which EB virus has become important, in providing them with a useful statement of the current situation. Even those directly involved in the large numbers of publications on the virus will welcome the presentation in one place of such a wide-ranging survey. □

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They touched the nerve of the Universe

V.F. Weisskopf

The Early Years: The Niels Bohr Institute 1921-1930. By Peter Robertson. (Akademisk Forlag, Denmark, 1979.) £6.

THIS book is a short account of the first ten years of Niels Bohr's famous Institute for Theoretical Physics. This institute was the scene of one of the most revolutionary and far-reaching developments in natural science, the birth of quantum mechanics. In some ways it was also the first international scientific institution, not in the sense of being internationally managed, but by having assembled, for shorter or longer periods, groups of young and productive physicists from many countries. Their names include most of the great leaders in physics such as F. Bloch, H. Casimir, P.A.M. Dirac, P. Ehrenfest, G. Gamow, W. Heisenberg, L. Landau, W. Pauli and many others. It was at that time, and with those people, that the foundations of the quantum concepts were created. In lively discussions the deepest problems of the structure of matter were

At the Institute early in 1931, Lev Landau, George Gamow and Edward Teller with two of Bohr's sons, Aage and Ernest.



conventional bonds, and a spirit of joy that can hardly be described by anyone who was not there at that time. In this great period of physics Bohr and his collaborators touched the nerve of the Universe. They were able to penetrate into the inner workings of nature that had been a secret up to this point. In the course of a few years only, the basis was laid for a science of atomic phenomena that grew into the vast body of knowledge known to us today.

The book by Robertson tries to recount the events that led to the foundation of that institution and the happenings during its first decisive and creative decade. It is the result of a painstaking research into the history of this unique institution. The author gives a lively and easily readable account in a little more than 150 pages, but one notices that he was not there at the time. We read about the early attempts to create a centre of international physics after the ravages of the First World War had interrupted scientific collaboration;

we read about the bureaucratic and financial difficulties that were overcome only with support by the Rockefeller Foundation; we read about the different scientists who came to the institute and about their work and their conversations with Bohr. One of the most attractive features are the many photographs of the institute, of the physicists, singly and in discussions, and of conferences and meetings that had taken place in Copenhagen and elsewhere during that period.

Reading this book and looking at the pictures is a nostalgic experience for those who had the privilege of being in contact with Bohr's institute in one way or another. I am not sure how much of the spirit and of the intellectual achievements of the institute is transmitted to a reader who has not had that great privilege. □

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brought to light. N. Bohr was the acknowledged leader; he created a style of thinking, the 'Kopenhageuer Geist'. The greatest among his colleagues, he was acting, talking and living as an equal among a group of young, optimistic, jocular, enthusiastic people, approaching the deepest riddles of nature with a spirit of attack, a spirit of freedom from

Ape language, cognition and culture

Carolyn A. Ristau

The Ape's Reflexion. By Adrian Desmond. Pp. 288. (Blond and Briggs: London; Dial Press/James Wade (Dell): New York, 1979.) £7.95, \$10.95.

THE APE'S REFLEXION is man; the possibility of the ape reflecting has created recent controversy. Is the ape capable of some primitive language; how extensive are his cognitive abilities; does the ape murder; does he have a sense of self and of death? Adrian Desmond deals with these momentous concerns in what I presume is intended to be a semi-popular book. His writing style is often fluid and he frequently achieves a sense of immediacy, seeming to

give an eye-witness account of the research. One can easily nestle down with his book for a few evenings' light reading. The sense of an eye-witness account is apparently not based on any visits by the author to research in progress. This is unfortunate, for an objective, careful and critical reporting of anecdotal information would be a useful addition to the literature on the ape language projects. The scientific literature suffers from a lack of such information while the popular literature abounds with dramatic and careless reports.

Briefly, Desmond's book concerns itself primarily with the ape language research projects, the neuroanatomical comparison of man and chimpanzee, tool use and proto-cultural activities of chimpanzee and early man, and some of Jane Goodall's investigations at the Gombe River Stream Preserve. There are numerous direct quotations from researchers' studies, an index and footnotes. The footnotes,

located at the back of the book, indicate references and sometimes offer detailed discussions. An alphabetical list of references and more such discussion would have been useful additions. The book also contains 24 pages of black and white photographs, predominantly of chimpanzees from the language projects and the Gombe River Stream.

Desmond considers man's close genetic relationship to the chimpanzee, citing 99% similarity in DNA nucleotides. Yet he does not note that the 1% difference appears to control timing and sequence of genetic expression, a mechanism by which organisms can develop very differently from each other. Furthermore, overall genetic similarity or dissimilarity need bear no necessary relation to the similarity of *particular* attributes of any two species. A dolphin is physically very different from man, yet *may* have certain cognitive abilities that are similar to man's. Desmond's comparisons between the Gombe River chimpanzee killing and man's act of murder, as well as many anecdotes from the ape language projects, are other examples of dramatic interpretations of data that need further analysis.

Desmond does raise important points. For example, in Desmond's consideration of the ape language work, Wittgenstein's aphorism, "If a lion could speak we could not understand him" (p.33), is quoted in reference to the naive assumption often made by trainers that chimpanzees use signs like 'please' and 'think' with the meanings assigned to them by humans (p.35).

Desmond also describes the recent investigation of Savage-Rumbaugh *et al.* (p.107) into the meaning of an utterance. Their chimpanzees seem to have the ability to use a lexigram or geometric design such as the design for 'key', in a highly specific context, but become bewildered upon its application to a wider array of contexts.

That is, the chimpanzees seem to recognize that use of the lexigram for key will initiate a chain of events that eventually leads to their obtaining food with a key, but do not seem to understand that the lexigram is the 'name of' a key. Gradually some kind of understanding closer to that level does seem to be achieved, but the research makes it clear that a chimpanzee's (or young child's?) appropriate use of a 'word' in a limited number of contexts does not imply understanding of the word.

However, as Desmond notes, words *can* 'map on' to thought. Premack conceptualizes his 'ape language' investigation as permitting apes to use words to map out conceptual structures the animal already possesses. The end of the work is thus not to teach an ape language or to wonder how linguistic the apes' abilities are or are not, but to use the ape 'language' as a tool for revealing some of the apes' cognitive capacities.

Throughout, Desmond wisely emphasizes that we cannot measure apes with a human yardstick.

Another of the more positive parts of Desmond's book is his exciting account of the trail of discoveries initiated by analysing soil samples and leading, finally, to the discovery, in the molecules of soil, of flowers at the grave of a Neanderthal man. Here was an indication of some ceremony for the dead, a remnant of culture in so early a hominid.

In all, Desmond does consider fascinating issues and offers some insightful thoughts. But one wishes at least for less wordiness, more dispassionate writing and less uncertainty about the validity of his interpretations. And does the reader need to find phrases like "Oh Darwin, where art thou now?" □

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Coulson's Valence

Linus Pauling

Coulson's Valence. Third edition. By R. McWeeny. Pp.434. (Oxford University Press: Oxford, 1979.) Hardback £17.50; paperback £8.50.

A SIMPLE but very powerful theory of chemical valence and the structure of molecules, especially of compounds of carbon, was developed in the period between 1852 and 1874 by E. Frankland, F. A. Kekulé, A. S. Couper, A. M. Couper, A. M. Butlerov, J. H. van't Hoff and J. A. Le Bel. It was extended in 1893 by A. Werner to include complexes of metals, which he identified correctly from chemical evidence as representing tetrahedral, square planar or octahedral coordination of ligands about a central metal atom. An important contribution to

the theory was made in 1916 by Gilbert Newton Lewis, who described the chemical bond as a pair of electrons held jointly by two atoms and who emphasized the stability of molecular structures in which each atom is surrounded by the same number of electrons as in a neutral noble-gas atom. Important contributions, such as the principle of electroneutrality and possible covalence as large as eight (for nickel in nickel tetracarbonyl), were made by Irving Langmuir from 1919 to 1921.

Despite some clarification introduced by the new ideas about the electronic structure of atoms and molecules, the theory of valence and the structure of molecules and crystals clearly remained incomplete. Many questions were asked in the first half of the 1920s that could not be answered; for example, why are the aromatic hydrocarbons more resistant to hydrogenation than other unsaturated hydrocarbons, why do complexes of palladium(II) and

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platinum(II) have the square planar structure whereas those of zinc(II) and cadmium(II) are tetrahedral, why are most compounds formed from the elements with a decrease in enthalpy, and most basic of all, why does sharing a pair of electrons between two atoms lead to a bond that holds the atoms together? The answers to these and many other questions were found in the period 1927 to 1933 through the application of the newly discovered theory of quantum mechanics to the problems. In 1927 ϕ . Burrau published his quantum-mechanical treatment of the one-electron bond in the H_2^+ molecule-ion describing the first molecular orbital, and in the same year E. U. Condon treated the electron-pair bond in H_2 by the molecular-orbital method, and W. Heitler and F. London treated the same problem by the prototype of the valence-bond method. In 1926, and in greater detail in 1931, I introduced the concept of hybridization of bond orbitals, and between 1931 and 1933 I discussed the quantum-mechanical basis of tetrahedral, square planar and octahedral coordination, ionic-covalent resonance energy, the electronegativity scale, magnetic properties of complexes, the structure and properties of aromatic and conjugated molecules, and several other matters relating to valence and molecular structure.

Between 1928 and 1931 the quantum-mechanical theory of the resonance of a

system between two or more possible structures and the consequent stabilization of the system by resonance energy had been discussed by John Slater, by me and by E. Hückel, and Slater had, in 1931, formulated his expression for the wave function representing an assigned valence-bond structure for a molecule. Many detailed quantum-mechanical calculations were being made of molecular properties by both the molecular-orbital method and the valence-bond method, and in 1931 both Salter and I published closely similar short papers to point out that, whereas these two methods in their simplest forms gave somewhat different results, when refined they approached each other, ultimately becoming identical.

During the 50 years since these events took place molecular quantum mechanics has made great strides. Many extensive calculations have been carried out, mostly with emphasis on molecular orbitals. I chose, however, to attempt to refine the theory of the chemical bond in a semi-empirical way, incorporating the ideas and principles of quantum mechanics. The results of this effort, in which I adhered as closely as possible to the ideas of classical valence-bond theory, were described in 1939 in my book *The Nature of the Chemical Bond* (third edition Cornell University Press: Ithaca, New York, 1960).

In the first edition of his book *Valence* (Oxford University Press: Oxford, 1952),

Professor Charles Coulson emphasized the molecular-orbital description of electrons in molecules, with rather little discussion of empirical information. This policy has been followed by Professor McWeeny in preparing the much enlarged third edition. The result is that the book contains only a small amount of information about valence. There is no significant discussion of oxidation number, the range of values of the valence shown by many elements, and a number of other aspects of valence theory. Instead, as the author states in his preface, the book provides an introduction to quantum mechanics and a discussion of the application of quantum mechanics to the structure of molecules, with emphasis on the molecular-orbital method (which the author describes as having largely superseded the valence-bond method). The level of the discussion is for the most part rather high, but there are some surprising exceptions, such as the failure to derive the wave functions for the hydrogen atom. The book provides a survey of molecular quantum mechanics and of some aspects of structural chemistry. An isolated student without previous knowledge of quantum mechanics might find it difficult, but it could serve well as the textbook in a course, the purpose for which it was written. $\square$

Linus Pauling is Research Professor at the Linus Pauling Institute of Science and Medicine, and is the holder of two Nobel Prizes.

Soviet innovation

Christopher Freeman

Science and Industrialisation in the USSR: Industrial Research and Development, 1917-1940. By Robert Lewis. Pp.211. (Macmillan: London, UK, 1979.) £12.

OVER the past 20 years the Birmingham Centre for Russian and East European Studies has established a high reputation for carefully documented research on Soviet R&D and industrial innovation. This book will certainly enhance that reputation and in many ways is even more interesting than its predecessors. Although it is entirely concerned with the inter-War years, the great interest of this period is that it was the formative time when the structure and procedures of the Soviet industrial innovation system became firmly established. It is also remarkable how many of the post-War debates in Western Europe were anticipated in the pre-War controversies in the USSR. Who remembered, for example, that the Rothschild "customer-contractor" principle was enthusiastically introduced in the Soviet institutes 50 years before its application here?

The new Soviet state inherited a science-technology system in 1917 which had many of the characteristics of today's under-developed countries — dependence on foreign technology and an almost complete lack of industry-based R&D. The Soviet leaders attempted from the outset to build up their own industrial R&D system, and the expansion in the 1920s and early 1930s was much more rapid than elsewhere. Indeed the Soviet Union has probably spent a higher proportion of its national income on scientific and technical activities than any other country for about half a century now. In absolute terms, this was, of course, still a fairly small total in the 1930s and as Robert Lewis is careful to point out "R&D" is only one part of the total rubric "science and technology". Definitions of R&D differ between Eastern and Western Europe and in any case were only formulated after the Second World War.

The first few chapters of the book document the growth of the Soviet science-technology system during the inter-War years with scrupulous attention to detail. (In a 200-page book, 50 pages are devoted to sources and methods.) The next few chapters review the organization of the system and the various attempts to control, coordinate and plan it. Finally, the last three chapters analyse this experience

critically with a particularly interesting discussion in Chapter 10 on "Yesterday and Today". They seek to answer the question: why has the return on the massive Soviet R&D investment been so unsatisfactory?

Robert Lewis concludes that the major structural weakness of the Soviet industrial R&D system was the failure to build up a strong R&D capability at the enterprise level and that this weakness persists to this day despite a succession of attempts to overcome it. Shortages of skilled manpower and equipment as well as the desire to avoid expensive duplication dictated the early decisions to concentrate the industry-related R&D in central institutes for each branch of industry. Ever since then, there has been a continuous theme of complaint that the central institutes were too remote from the real problems of the factories and conversely that industry had neither the capacity nor the incentive to apply the results of the R&D conducted in the central institutes.

This problem is by no means confined to the Soviet Union or to centrally planned economies. Even within large private enterprise, it is a familiar theme of the R&D management literature. In one form or another it is likely to persist even in the best-managed systems as it reflects a real difference in preoccupation and pers-

pective between those primarily concerned with the management and improvement of an existing production system and those concerned with potential changes in products and processes including the total displacement of existing systems. However, there seems little doubt that the problem was particularly acute in the Soviet Union as institutional barriers were reinforced by geographical and political constraints. Lewis shows that, despite many declarations of good intentions about decentralization, the R&D establishment remained overwhelmingly concentrated in the Moscow and Leningrad regions. He calculates that a decade of pressure resulted only in a reduction of the proportion employed in these two regions from 80% to 75%. Again, this is by no means exclusively a Soviet problem.

The final chapters suggest, although not very explicitly, that the most important specific Soviet weakness has been the over-centralization of decision-making associated with Stalinism. It is of some interest that already in the 1920s Pyatakov and other members of the "Left" opposition were advocating the wholesale decentralization of industrial R&D to the enterprise level. The combination of the output target system, the import of 'turn-key' foreign technology, the shortages of skilled people and the decimation of the intelligentsia in the purges of the 1930s meant that the various attempts which were made to strengthen enterprise-level R&D had little effect.

The major exception which Lewis points to was the aircraft industry, and the discussion of its peculiarities in Chapter 9 is one of the most interesting in the whole book. This industry was a high priority for the leadership throughout the 1920s and 1930s which found expression both in a relatively lavish level of funding and in a readiness to change the organizational system in order to get the results so urgently demanded by Stalin and other leaders, who apparently showed an almost daily interest. Whereas in other industries it was difficult to get adequate resources for prototype and pilot plant work, in the aircraft industry several alternative designs were often promoted simultaneously right through to the prototype test stage in development. In the aircraft industry too, the single central research and design institute gave birth to a pluralistic structure already in the early 1930s which included the rather successful flexible design groups assembled under the leadership of individual designers and sometimes located at factories.

Centralization to a high degree certainly persisted in the aircraft industry, but it was combined with a real choice in design and development based on autonomous effort and with relatively good communications with the production systems. An interesting point made by Lewis is that sometimes factory personnel were brought into the design and development teams at a fairly

early stage. He does not discuss it, but one may guess that this industry also made good use of its own products to ease the physical communication problems of the USSR.

By preventing Germany and Japan from embarking on expensive aircraft, nuclear and other military R&D projects for two decades after the Second World War, the USSR and its War-time allies conferred upon them the inestimable blessing of concentrating most of their best engineering and scientific talent on the industries which really mattered for economic growth and international trade performance. The results are now increasingly apparent. The British in a mild way since 1945 have suffered from the Russian problems of the 1930s — too high a priority for R&D in prestige defence-related industries and relative neglect of the

rest. As in the USSR, a very high proportion of our best science and engineering talent was locked into this system for 30 years, while the mechanical engineering industries, vehicles and other mundane branches of the economy did not have the capacity to design and develop new products to keep pace with world competition. To this day, they do not have and often cannot get the electronic engineers they need to re-design their products and processes. It would be ironical if the 'Iron Lady' in her anxiety to combat the Soviet 'menace' caught a more virulent strain of the Russian 'flu'. □

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Voyages through the cosmic landscape

D.D. Clayton

Cosmic Landscape: Voyages Back Along the Photon's Track. By Michael Rowan-Robinson. Pp.149. (Oxford University Press: Oxford and New York, 1980.) £4.50.

WHEN I began this book I was somewhat prejudiced against it because I thought it would sink from the burden of its seemingly unnatural standpoint. After all, astronomical knowledge has exploded from the combined measurements of astronomical objects by instruments responsive to the different spectral bands. Yet, here Michael Rowan-Robinson presents "cosmic voyages in each of the main wavelength bands of contemporary astronomy". He finds himself "taking this metaphor of voyage literally, and describing how I think things would look from a viewpoint different to that seen from earth. Certain objects in the cosmic landscape figure in several different wavelength bands and so our picture of them builds up only gradually from the succession of voyages in each band". Surely, I thought, the only way to discuss the Crab Nebula, or QSO 3C 273, would be from the simultaneous knowledge from all types of measurements, and not from a succession of half-blind glimpses. But in the end my negative predisposition was overcome by an informative and often poetic presentation. This is a very good book, although it fails by slim margins to be a great one.

The author says, "I have tried to write a book for someone who might never have read anything about astronomy". So restricted, he must at times be a teacher: "What is light?"; "What is colour?"; "How is light made?"; "What are radio waves?" are subsections of chapters in which he must also explain such phenomena as atomic and molecular structure and radioactivity without seeming to delay forever the departure of the cosmic expresses. Amazingly, he has surmounted this obstacle with as much grace as one can hope for. He does this by appealing to general matters of human interest — for example, by describing briefly the red, green and blue-light-sensitive organic pigments in the cone-shaped detector on the back of the human retina. Occasionally, he forgets this audience and uses technical terminology that can only impede the novice; for example, because he uses it he must explain that in writing O VI "the Roman numeral corresponds to one more than the number of missing electrons". The book is, I think, a bit difficult for readers without scientific backgrounds; but the author has made a good compromise between simplicity and the inspiration of real intellectual content. For this reason, I will recommend this book to nonprofessionals interested in astronomy, even knowing they will not understand some of it.

So we set off in succession on six voyages: the visible landscape; the radio landscape; the ultraviolet landscape; the X- and γ -ray landscape; the infrared landscape; and the microwave landscape. I could not discern the author's reason for this order; but neither can I criticize it as illogical or inappropriate. Perhaps putting the infrared landscape off until the fifth voyage has something to do with the author's personal research emphasis. It warms him especially to this voyage, causing him to note here, almost sadly, that totally new surveys of the sky seem to be

now a part of history, and then to the confession: "For me it has been a wonderful time to be alive and to be an astronomer. And so I try to share this experience with you".

To analyse the success of this book I must, like the author, branch out from astronomy into the larger realm of human experience. A landscape is interpreted as a partly subjective human experience, with the human at its centre. Its objective, in an artistic sense, is not the description of the separate parts of the scenery, but rather a description of the act of perceiving it. Rowan-Robinson has not idly chosen a catchy title; he has chosen it specifically to his artistic purpose. The ponderous structure of separate voyages in different wavelength bands succeeds because his purpose is not to explain our understanding of the objects in the landscapes, but rather the human act of perceiving them. He evokes, perhaps unintentionally, the experience of the child in its crib, looking out at baubles hanging above him and wondering at the meaning of a perception he cannot touch. It is a powerful undercurrent that gives

force, by its repetition, to these cosmic voyages. The reader glimpses the human race in its infancy, reaching out.

I cannot say that Rowan-Robinson intended all of this, but I believe that he did. As someone who has also attempted to enlarge the genre of scientific writing, I can only admire his effort. It falls short of greatness because the author was not sufficiently single-minded in this task. The pedagogical explanations weigh down the poetic experience. We have here the artist painting his landscape and, at the same time, explaining to us how we know that the water in the creek flows downhill rather than up. On the positive side of that coin, however, there is fascinating astronomical and physical knowledge interestingly described on each page. So it is also a book from which to learn about astronomy. For a skilful blend of these objectives, the author earns grade A. I remain fascinated with the partially fulfilled opportunities of this book as a poetic and artistic experience. I can best share this fascination by the following poem, which this book inspired me to write:

On His Reading *Cosmic Landscape*

Hanging there on strings, tiny baubles above my crib,
What would I name you if I, like they, knew words?
Jupiter, Venus, pulsar, quasi-stellar objects of my gaze,
Abstaining from warm flesh from which I grew,
You surely breathe and eat and cry like I.
For what can I now know of death, of that passage
That will one day threaten the incomprehensible world?
Silently they hang above me, beyond my reach, unreal . . .
Unreal at least except for eyes, mystical extensions,
Touch without fingers, motion without impact, something there,
But maybe not important, milkless, without warmth.
Still, you must have a name, a kind of category,
Something that can let me live at peace with you.
Could I but sense you in my way, some measure perhaps,
Could I but bridge the gulf that lies between,
Maybe I would do so, but how? what do I need?
A name is fine, but will it shore up liberty
From ignorance, from lack of touch, from measurement?
From measurement! What new idea now swarms within me?
Perhaps a mathematics, calculations from untouchables
Can clarify your orbit, predicting perigee perhaps or self collapse.
Unending accusations drain this dream: worthless metaphysics,
Not a method to trust, yet my heart does beat to it.
Have ancient genes bestowed on me this thrill,
Forgotten values to mankind accrued to permanence in me?
Urging explorations, voyages from this nursery,
They inflame my nerves where comfort never can.
Rattlesnakes see infrared and suffer no abject demise,
Bats somehow hear a soundless noise, compressibilities,
But all the same to me, here, not knowing, ignorant,
Wondering simply what it means to see beyond,
And if this oh-so-childish room will stand the test
Of time, when I no longer am its nexus.

D.D. Clayton, 1980

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An optimist's selection of nightmares

Peter J. Smith

A Choice of Catastrophes. By I. Asimov. Pp. 377. (Simon and Schuster: New York, 1979.) \$11.95.

IMAGINE a small black hole about the size of the average asteroid and with a mass of, say, a millionth of that of the Earth. Suppose then that this remarkable object were to enter the Solar System and score a direct hit on the Earth. What would the result be? The "mini-black hole" would tunnel its way into the planet, absorbing matter and thus growing as it went. If it had sufficient momentum to begin with, it would burn a path right through the Earth, causing enormous explosions where it entered and left. At the points of entry and departure life would stand little chance of survival, although the Earth itself would probably heal and go on as before.

But what if this mini-black hole had too low a velocity to enable it to pass through the Earth and instead became trapped in the terrestrial gravity field? It would then fall towards the planet's centre, overshoot, fall back, overshoot again, fall back again, and so on. Because of the Earth's rotation, however, the oscillation would not be along a single line but along a series of linked tracks. The mini-black hole would therefore carve out an irregular volume from the Earth's interior, growing larger as it did so and absorbing more matter with each sweep. Thus hollowed out, the Earth would probably collapse into the black hole with a diameter of 2 cm and a mass and gravitational field identical to that of the planet it had destroyed.

Fantasy? Possibility? Probability? No, yes, no — in that order. As it happens, mini-black holes, black holes of less than stellar size, have not yet been detected, although they were postulated on theoretical grounds by Stephen Hawking in 1974 and to that extent are a serious scientific possibility. The chance of one of them colliding with the Earth must be regarded as remote, however, if only because if such hole-planet collisions were common there would be far fewer bodies in space than there actually are. On the other hand, annihilation of the Earth by a mini-black hole is only one of a bewildering variety of possible catastrophes, many of them less bizarre than that, through which the human species might meet its end.

Indeed, there are so many possibilities here that in his book about them Asimov has had to design a five-fold classification system in order to make the overall picture intellectually assimilable. To begin with, then, we have "Catastrophes of the First Class", defined as changes such as to

render the whole Universe uninhabitable. One of these — the so-called heat death, by which all non-thermal forms of energy are reduced to heat, and entropy reaches its maximum — is inevitable, although it will not occur for at least a thousand billion years. Equally inevitable, though on an even less certain time-scale, is the contraction of all stellar bodies to black holes and the ultimate coalescence of the whole Universe into a single, huge black hole. And finally there is the possibility that the Universe will contract to the sort of 'cosmic egg' from which it developed via the big bang, although this depends on whether the Universe is open or closed (possibly oscillating).

Inevitable or not, however, all First Class death threats are too far ahead to worry about; they are intellectually interesting but of no practical concern. The same could be said about most "Catastrophes of the Second Class", namely, those able to destroy life on Earth by killing the Sun, leaving the rest of the Universe intact. Ultimately — in billions of years time — the Sun will develop into a red giant and then white dwarf, ending its role as supporter of life on Earth. Moreover, in the shorter (but still very long) term the Sun could be annihilated by collision with a star, a 'normal' black hole or a large body of antimatter (which may or may not exist), all of which in any case should give plenty of warning.

With "Catastrophes of the Third Class", on the other hand, a new factor enters — the possibility (but improbability) of immediate death. An undetected mini-black hole of the type mentioned earlier could even now be about to enter the Earth's atmosphere, interaction with which would give a three-minute warning of the planet's annihilation. But if that sounds alarming, it has to be admitted that most other Third Class changes, defined as those potentially able to destroy all terrestrial life by disturbing the Earth itself, are unlikely actually to do anything of the sort. Collision with extraterrestrial objects (asteroids, meteors etc.), interaction with the Moon, earthquakes, volcanoes, moving plates, glaciation and a disappearing geomagnetic field all can, and do, give rise to local disaster, and some (especially glaciation) could conceivably destroy modern Western civilization; but only in very exceptional circumstances could they obliterate all life, or even all human life.

Which brings us to "Catastrophes of the Fourth Class", or those capable of destroying all human life but not most other life forms. This category includes being overrun by insects or rodents, war, the spread of infectious disease and attack by superior extraterrestrial intelligence, although war (especially thermonuclear) and a hitherto unknown disease (perhaps man-made) seem to be the only two worth worrying about. Perhaps of rather more concern, however, are those activities and

events unlikely to destroy all human life but able, nevertheless, to ruin civilization as we know it — "Catastrophes of the Fifth Class". War again, depletion of natural resources (including energy) and pollution are the chief contenders here.

Asimov is an optimist, evidently believing that those catastrophes possible in the short term are avoidable with intelligent handling, although he warns that success in overcoming them could lead to new dangers such as overpopulation and starvation. Be that as it may, he has left us in the meantime with a remarkable survey of the possible dangers, ranging from the

fascinatingly bizarre to the frighteningly realistic. His book, though by no means short, manages to pack an astonishing number of explanatory asides (on entropy, quasars, red giants, supernovae, DNA, cosmic rays etc.) into a story covering an equally remarkable number of primary disciplines (cosmology, geology, biology, technology etc.). I could quibble a bit over points of presentation, but there is no denying Asimov's explanatory powers. □

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Theology after Darwin

A.J. Cain

The Post-Darwinian Controversies. By J.R. Moore. Pp.205. (Cambridge University Press: Cambridge, 1979.) £18.

THE subtitle of this book is "A Study of the Protestant Struggle to Come to Terms with Darwin in Great Britain and America 1870-1900", and indicates its nature far better than the main title. As such, it is not in one sense a book to review in a scientific journal, although *Nature* did publish an exchange of letters between Asa Gray and G.J. Romanes on the religious implications of evolution, in 1883. In another sense, as a study of the intellectual forces resisting, modifying, or encouraging the spread of a particular complex of ideas, it is of interest to everyone.

An idea or complex of ideas is introduced into so heavily structured a space, and engenders such complex reactions, that any account of its spread can only be made, as yet, historically, philosophically or bibliographically. I once consulted a professor of plasma physics on the mathematics necessary for modelling the spread of an idea; he dismissed the subject out of hand as "too hopelessly complicated". And as any historian, philosopher or bibliographer has himself a mind already structured, his account will necessarily be slanted to some extent. Slight slanting is insidious; blatant slanting usually produces a rival account, useful or merely eristic.

James Moore is particularly concerned to do away with the old metaphor of warfare between ideas — the victorious army of the Darwinians, the forces of obscurantism in full flight, the signal victory of Huxley over Wilberforce, and so on. It cannot be denied that there were plenty of people who belonged to neither 'army'; they accepted doctrines of both

camp, and struggled to reconcile them. Two chapters of the book are entitled "Warfare's toll in historical interpretation" and "Towards a non-violent history". These are in Part I, "Historians and Historiography", in which Moore points out the evils of such a schematic representation of extremely complex events. He blames John William Draper (*History of the Conflict between Religion and Science*, 1874) and Andrew Dickson White (*A History of the Warfare of Science with Theology in Christendom*, 1896) particularly for the prevalence of the metaphor in the evolutionary context, giving very useful sketches of their careers and intellectual conflicts to explain both the nature of their books, and the wide difference in scholarship between them. In Chapter 4, "Towards a non-violent history", he brings out his own approach, deploring the use of the military metaphor as showing "the absence of any deep moral aversion from war". Yet he himself allows (as any man of sense must) that "Christians in the late nineteenth century were beset with spiritual disorders and intellectual strife". The metaphor of armies of individuals fighting may be unsatisfactory; at this point the reader might think him about to take up that of gladiatorial combat between ideas within a single individual, which at first sight is nearer the truth. But it is far more useful, as Moore shows, to take a less superficial view. He uses Festinger's theory of the structure of cognitive conflict to show how the individual (merely the arena in a gladiatorial metaphor) is active in reducing the dissonance between incompatible ideas, either by altering one or other idea, or by introducing new ones that reduce the dissonance. He illustrates the point pleasantly by a flat-earthier confronted by an extra-terrestrial photograph of the Earth, and more seriously by the example of that unfortunate man St. George Jackson Mivart. Violence, then, is replaced by the reduction of dissonance — an undoubted gain, but one cannot help feeling that a real baby (incompatibility of ideas leading to real distress) was nearly

thrown out with a lot of bathwater.

Moore gives a useful sketch in Part II, "Darwinism and Evolutionary Thought", of the actual scientific and philosophical issues at work in his period, showing Darwin's difficulties, methodological criticisms, the influence of Herbert Spencer, and the differences that transformed Darwin's Darwinism into neo-Darwinism. (His account of the influence of Paley on Darwin, later in the book, is especially valuable.) Unfortunately, he adopts Morse Peckham's distinction of Darwinism and Darwinisticism — Christian Darwinism "understood Darwin's theory and left it substantially intact, neither emasculating it nor adulterating it with foreign ideas in the interests of dissonance reduction" and Christian Darwinisticism (unpleasing term) "either misunderstood, misinterpreted, or modified Darwin's theory, adulterating it as they had need with non-Darwinian ideas". (Surprisingly, these definitions are not in the index.) It comes as a shock to the student of evolution to find Lamarckism labelled Darwinisticism.

Lastly, in Part III, "Theology and Evolution", Moore makes his most valuable contribution, with sketches of 28 Christian controversialists, American and English, their intellectual predispositions, development and final attitudes. Most are substantial figures, well worth analysing. A few are more reminiscent of Elderess Polly, Elderess Antoinette and Newman Weekes, in Matthew Arnold's bland and devastating account of religion in America. Moore shows that some more orthodox Christians, especially Calvinists, had far less difficulty in accepting natural selection and the struggle for existence as the true cause of evolution than did most liberals, and that much of what has been written on the period shows a complete lack of understanding of Protestant stances.

It would require a far more massive exposition even than Moore's to do justice to these great themes. Moore has much to say on progress, providence and criteria of explanation. Yet his treatment of theological themes omits, rather surprisingly, all useful mention of the Fall, and not too much is said of the creation of Adam and Eve. The theologically orthodox positions he discusses (and the reader is sometimes not clear about *which* orthodoxy he is discussing) are mainly of Dissent; Catholics and Anglicans feature prominently but are not as well analysed as Congregationalists, Unitarians or Presbyterians. A more serious weakness is superficiality in the analysis of some of his characters. For example, Frederick Temple is commended (rightly) for his "generous and incisive" sermon to the British Association the day after the famous encounter of Huxley and Wilberforce, and his *Relations Between Religion and Science* (1885) is quoted as reducing natural selection to "one partial expression" of the

original properties impressed on matter by the Creator. (This, of course, is the old fallacy that if you can write an equation for something, then the equation being devoid of emotions, so should we be in contemplating the thing.) Moore also quotes Aubrey Moore's correct criticism that Temple's attitude although he was an Anglican clergyman (and later Archbishop of Canterbury) was pure Deism, not Christianity. But he does not point out that Temple's *Relations* is one of the worst examples published of using the then ignorance on certain scientific subjects to insist that God must have acted directly in these matters. Huxley's correct criticisms of Lord Kelvin on the age of the Earth disproving evolution by natural selection

are dismissed in quoted words as to be "praised more for their vigor than their strength", although a divine making the same point is referred to without qualification. (It is not always clear whether the book is an analysis of attitudes, in which case it should be more trenchant, or a demography, which requires a greater coverage of people.)

This book is a 'must' for historians of ideas, useful for students of evolution and theology, and quite interesting enough to recommend to the public generally. □

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Better buy a pair of climbing boots

Fred Dainton

Scientific Productivity: The Effectiveness of Research Groups in Six Countries. Edited by F. M. Andrews. Pp.469. (Cambridge University Press: Cambridge, UK, and New York; UNESCO: Paris, 1979.) £20.

REMINDED by the title of this book of currently fashionable productivity deals in wage bargaining and on the evidence of a photostat of the title page, the list of comments and the editor's explanation of the purpose of this book, I undertook to myself to review it in a matter of two or three weeks even though I have a rule that all reviewers have an inescapable duty to read every word the author puts before them. I failed in the task not because of any physiological inability to reproduce on my retinas faithful images of the words in front of me nor because of any lack of interest in the subject (wouldn't we all like some philosopher's stone which would enable us to improve the productivity of research units?), nor because of any unwillingness to learn the techniques of the sociologist, so abundantly deployed here, if that were the necessary price of wisdom, but simply because of a great weariness of the flesh induced by perusal of the pages.

As I hacked my way through the verbal undergrowth, pausing to absorb the significance of each of the numerous qualifying clauses, worked my way through complicated diagrams and then re-read in order to make sure that despite all appearances there must be gold somewhere, I longed for the experience, common sense and humanity of a Medawar to tell us in plain words how

scientific productivity can be increased. My prayers were answered for on the 28th February his *Advice to a Young Scientist* was published and happily fell into my hands, and, though this latter book contains no figures, no tables and less than one-fifth of the verbiage of *Scientific Productivity* and is not explicitly directed to this subject, Medawar has far more of value to say to scientists, scientific administrators, and science policy makers and watchers at one-quarter of the cost.

Scientific productivity is a difficult concept, raising questions of volume, intellectual or experimental excellence, magnitude and nature of the impact of the research unit's output on the development of the subject (some developments seen as exciting at the time they are published are later shown to be inhibitors rather than catalysts of progress, a point not really brought out in the book), applicability to economic or social ends etc., etc. Moreover the weight to be attached to each of these many factors might be expected to depend not only on time but on the social and political viewpoint of the assessor. So one has great sympathy with the team of investigators in their methodological difficulties. Therefore the reader tends to concentrate on the conclusions to see if they are so much better and more useful than widely held views and opinions that, as that rock of common sense Ralph Waldo Emerson predicted "The world will make a beaten path to his [in this case the authors'] door". I fear the world will not, for who will want to read so much to arrive at conclusions like the following. I quote from Part 1: "The social position of the individual within the social hierarchy of a research unit proves to be one important correlate of differences in performance at the individual level and the size, age and scientific exchanges of the research unit are additional factors that relate to group productivity", "The results from academic research units seem to be in accord with the 'human relations thesis' that is, that the idea that good leadership

leads to a high group morale and that high morale leads to increased productivity by group members", "that human resources are more significant than [financial resources or access to information]", "The results show that higher levels of effectiveness tend to occur where there is more communication" and "That higher performing units tended to have more dedicated professional members with more diverse working roles and intellectual resources".

But one must have some sympathy for the authors. It seems that a questionnaire, reproduced in an appendix, was sent out to a large number of workers in many European research units. 11,000 of them in 1,200 units distributed over six countries responded. Those who devised the questionnaire are also the authors of the 14 chapters which comprise the book and they had at their disposal all the information elicited by the questionnaires. Apart from the first two chapters (in many ways the best in the book) describing how the International Comparative Study on the Organisation and Performance of Research Units was conceived and mounted and the resultant data analysed, each chapter is by a different author or group of authors. Full freedom was given to each to use his or her own methodology and to exercise personal judgements, and no attempt was made to draw them into a symposium at which differences and common features of their conclusions could be collectively explored. Indeed there seems to have been a perverse pride in avoiding this useful device and also in inexplicably arranging the chapters in an order "alphabetical by the authors' national or international affiliation". I expect that, like myself, other sociologically illiterate scientists would have been grateful to have had some guidance provided by a consensus of the consortium members as to the weights to be attached to the various findings. Nonetheless several chapters do have some useful reviews of past work, and some conclusions are drawn from this and from the empirical data yielded by the enquiry.

It is sadly ironic that the book appears under the imprimatur of Cambridge University where so many highly successful research units have been established either by administrative fiat or more often have evolved from a happy coincidence in time and place of an opportunity and those capable of seizing it. This prompts the questions as to whether Max Perutz, when building up and running the highly successful Laboratory of Molecular Biology in Cambridge, and indeed others with similar track records, would have done better if they had had all these conclusions firmly embedded in their consciousnesses and, more importantly, whether those charged with the responsibility for establishing units in the future would make better decisions after reading this book. The answers must

remain a matter for conjecture but I am quite certain that they would have been even better advised to read Medawar and then ask themselves a series of questions: for example "Would a research unit in this field offer a reasonable prospect either of a significant advance in knowledge or of meeting some societal need? How can I identify and appoint the highest quality person(s) to lead it and procure the necessary material resources for them? What is the best intellectual environment for the unit to flourish?" And perhaps of greater importance "How can the inevitable tension between the paymaster and the research unit be mediated so as to maximize the freedom of the Director and his staff which is necessary for high creativity and productivity without giving them a licence grossly to ignore the terms of reference under which they operate?" Then there is finally the most difficult question as to how one is to know when the research unit has served its purpose or has lost its quality and should be closed. There is little in this book which helps with the resolution of these practical problems and what there is is difficult to quarry without reading the whole book for there is no subject index.

Of course this is not the only book which fails such tests of practical utility to the decision makers but which may nonetheless be of value to the community of scholars to which the authors belong, in this case all those interested in sociological enquiry. I am not competent to express any opinion on this latter question, and in any case this is perhaps of less relevance because the Foreword by UNESCO makes it plain that the work was the outcome of that body's Nineteenth General Conference and the same Foreword refers sympathetically to "... citizens in many countries asking for more public accountability. They want to know the use to which their resources are put and they demand that the programs that they support shall be both efficient and effective". That these desires should be gratified is unexceptionable. This principle can equally be applied to the UNESCO programme of which this book is the visible and ponderous product. All the more pity that we are not told how much the whole enterprise cost and thus be able to judge whether the expenditure was justified. I have a strong feeling that if those scientific directors of research units who spend £20 on this book were each to buy, at slightly greater cost, a pair of good mountaineering boots and, after salubrious daily scrambles, to confer on this topic in the evenings throughout a week spent at some delectable Alpine centre they would bring down from the mountain more useful tablets than this book. □

Sir Fred Dainton is Chairman of the British Library Board, London, UK.

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Coats of many colours

Anne McLaren

The Coat Colors of Mice. By W.K. Silvers. Pp.379. (Springer: Berlin, Heidelberg and New York, 1979.) DM59.60, \$32.80.

SOME targets of scientific study are inherently attractive. One such is soap bubbles. Another is coat colours and patterns. In the days when I knew R.A. Fisher, the father of modern statistics, his true pride and love was for mouse genetics, and to be asked the effect of umbrous on an agouti pink-eyed brown background would really make his eyes twinkle.

W.K. Silvers' book is also a labour of love. There is no mention of Sturtevant's intriguing suggestion that Mendel discovered his 'laws' of segregation and assortment by illicitly crossing different-coloured mice in his monastery cell, later using his curiously exact pea ratios as a mere didactic demonstration; but in other respects the early history of the subject is well covered. The references include more than 20 papers published in the first two decades of this century on the genetics of coat colour in mice, starting with the classic 1902 paper of Cuénot. One of these pioneers is still living, Naomi Mitchison, who, with her brother J.B.S. Haldane, published in 1915 the linkage between albino and pink-eyed dilution, the first to be established in any mammal. A start was also made in those early years on the chemistry of mouse pigmentation (Onslow, 1915) and on the developmental analysis of lethal yellow (Ibsen and Steigleder, 1917), while C.C. Little's life-long interest in mouse spotting genes gave rise to a series of papers beginning in 1913.

Moving to more modern times, Silvers pays tribute to Grüneberg's monumental *Genetics of the Mouse* (Nijhoff: The Hague, 2nd edition; 1952), to Searle's *Comparative Genetics of Coat Colour in Mammals* (Logos Press: London; 1968), to Mintz's illuminating studies on experimental chimeras ('allophenic' mice) and to the great amount of assistance that he received from Rita Phillips, whose very recent death is a sad loss to mouse genetics as well as to her many friends and colleagues.

Apart from its aesthetic appeal, the study of coat colours embraces not only genetics (148 alleles at 63 loci), but also biochemistry, cytology, histology, anatomy, embryology and endocrinology. Silvers does justice to all these aspects. The first half of the book, in which he deals with the various colour-controlling loci and their complex interactions, is perhaps more satisfactory than the second half, which covers spotting genes, chimeras and X-chromosome mosaics. The etiology of

spotting and coat colour pattern is still widely debated, and even the experimental evidence is sometimes conflicting. Silvers does his best to summarize the literature, but the picture that emerges is patchy in more senses than one. For example, in the field with which I am most familiar, he perpetuates confusion by failing to make clear that Mintz's 'standard' or archetypal pattern is a valuable abstraction rather than an observed pattern ('observed relatively rarely' is misleading when the true probability, for one side of the mouse alone, is 2^{-16} , less than 1 in 100,000); he does not mention Lyon's interesting suggestion that the frequent brindled sectors of chimeras arise because each territory may be colonized, not by one but by two 'primordial melanoblasts', of the same or contrasting genotype; and he affords the same status to the clonal theory of pattern formation for hair follicle chimeras as for melanocyte chimeras, though the evidence is far less cogent for the former than for the latter.

But these are minor grumbles. The book as a whole will be a standard reference work in every mouse genetics laboratory for many years to come. Not only does it make admirably clear the complex interplay of genetic (and in some cases environ-

mental) factors in determining coat colour, but it also uncovers areas where further research is badly needed. These include spotting genes, of course; the intriguing behaviour of silver, resembling the effects on pigmentation of irradiation, and perhaps also ageing; the 'reds' of the mouse fancy, and no doubt other fancy types also, not yet analysed genetically; and, above all, the need for a codification of colour, a decent set of colour standards, which although doubtless imperfect could hardly but improve on such phrases as "a dull brown colour", "a medium shade of sepia", "a faint cream or ivory color", "a brownish shade, a little lighter than an ordinary pink-eyed brown with a slight dull yellowish cast". The three colour plates in the present volume are not of very high quality, though better than nothing; really good colour reproduction would have presumably escalated the price unacceptably.

As it is, the book at about £15 is reasonably priced, there are good author and subject indexes, and the bibliography is comprehensive. □

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The classic electricians

W.D. Hackmann

Electricity in the 17th and 18th Centuries. By J. L. Heilbron. Pp. 606. (University of California Press: Berkeley, Los Angeles and London, 1979.) £24.

AN ambitious work that deserves to become a classic in this subject. Not only has Professor Heilbron incorporated all the historical papers on early electricity that have appeared since I. B. Cohen's *Franklin and Newton An Inquiry into Speculative Newtonian Experimental Science and Franklin's Work in Electricity as an Example Thereof* (The American Philosophical Society: Philadelphia, 1956), but he has also included much new material, such as the importance of the seventeenth century Jesuit polymaths in the study of physics in general, and in electricity in particular. For those of us reasonably well versed in this subject, the main developments as given in this book are known, but it is useful to have such a broad and up-to-date survey. The general reader interested in the history of physics, or of electricity, should find this a fascinating story, although he might miss some of the finer philosophical or technical nuances. Any historical treatment of a scientific subject must contain inevitable

distortions as all is focused on specific aspects of the total picture, but by attempting such a broad canvas, Heilbron has managed to keep these distortions to a minimum.

A work of this nature, covering such a large time span, is inevitably faced with historiographical problems. Terms and concepts change their meaning, brought about by new discoveries and by changes in philosophical outlook, and this makes interpretation difficult. The author has to make it clear to the reader that a problem dealt with in the seventeenth century was handled differently from the same problem looked at by physicists one hundred years later. When surveying a subject over a very long period, it is possible to give it a false appearance of continuity, while the subject progressed (or changed) in a much more convoluted manner. In the context of this book, both the concepts of physics and of electricity changed dramatically during these two hundred years. The word 'electricity' was to undergo subtle changes in meaning during the century after its use by Sir Thomas Browne in his *Pseudodoxia Epidemica: or, Enquiries into very many received tenents and commonly presumed truths* (1646), causing endless confusion to historians of science in the past. At first it simply referred to the attractive property of amber and other substances. The word only began to denote a separate entity or substance causing the electrical behaviour of attraction and repulsion during the early 1740s. Heilbron has coped admirably with this kind of problem, not least by the way

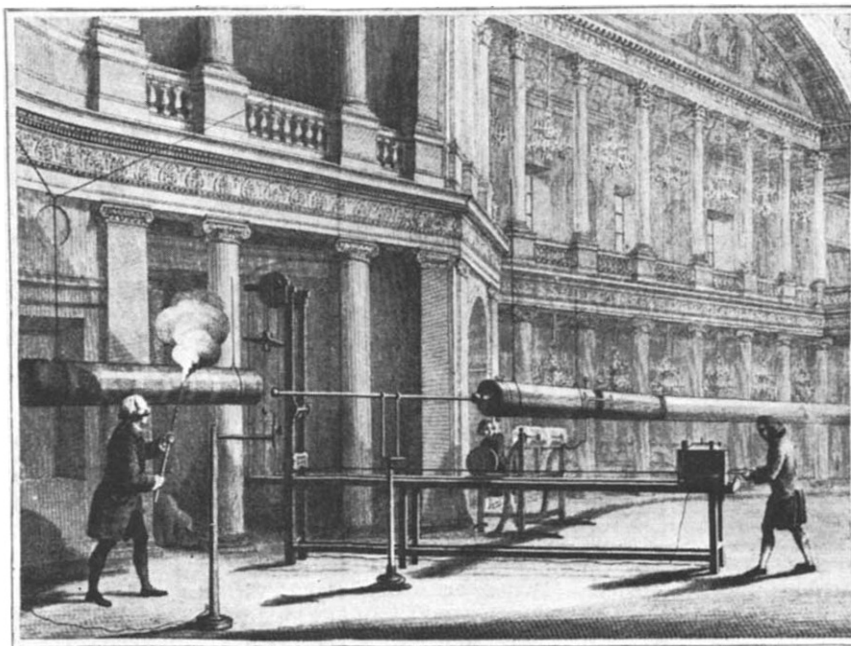
he has structured this book, which he has divided into five parts.

Part 1 sets the scene by giving a general survey of early modern physics, its underlying principles and the ways in which it was diffused, including the founding of scientific societies, the development of better instruments and the importance of lecture-demonstrations. He divides some 210 electricians (as those studying this subject called themselves) into five groups according to their chief means of support: members of religious orders (especially Jesuits), paid academicians, professors, public lecturers, and others (primarily instrument-makers, doctors, lawyers, clergy and the independently wealthy). Between one-quarter and one-half were professors of universities or secondary schools. It is difficult to determine how approximate these figures are. They contain overt generalizations (of which the author is aware), and there are inevitably minor inconsistencies and errors. For instance, in the table dealing with the number of academies founded between 1660 and 1800, he lists no provincial societies under Britain (apart from one by name), while under 'others' in the case of France, he lists 27, and under Holland 14 (these include agricultural societies). This produces a distorted and not very meaningful picture. In general, however, his conclusions are justified.

Part 2 surveys the progress of electricity during the seventeenth century, starting with Gilbert's experiments on the amber effect in *De magnet* (1600), terminating soon after Otto von Guericke's experiments with his 'globus mineralis' in the 1660s. Part 3 covers the first half of the eighteenth century, starting with the researches of Hauksbee the elder, at the Royal Society during the early 1700s. Part 4 deals with the impact of the Leyden jar (a primitive condenser), discovered in 1745, and the reception of the theories of Benjamin Franklin. Part 5 covers the last thirty years or so of the eighteenth century, during which the main electrostatic principles were finally formulated and attempts were made at quantification. This period is terminated with an Epilogue describing the invention of the voltaic pile or battery in 1799. During the entire two hundred years, 'electricity' meant electrostatics, then known as 'frictional electricity', as it dealt with charges generated by rubbing glass or other electrics.

The sub-title, 'A Study of Early Modern Physics', is significant, for the author has tried to relate the history of electricity to the broader conceptual changes that occurred during the rise of early modern physics. Effectively, he has divided the history of this subject into three stages. The first stage, which lasted until 1700, was still dominated by Aristotelian physics, a textbook science which included organic and psychological as well as inorganic phenomena. The second stage, lasting until

A View of the Apparatus and part of the Great Cylinder in the Pantheon, 1777.



Wilson's public model lightning conductor experiments at the Pantheon, London, 1777.

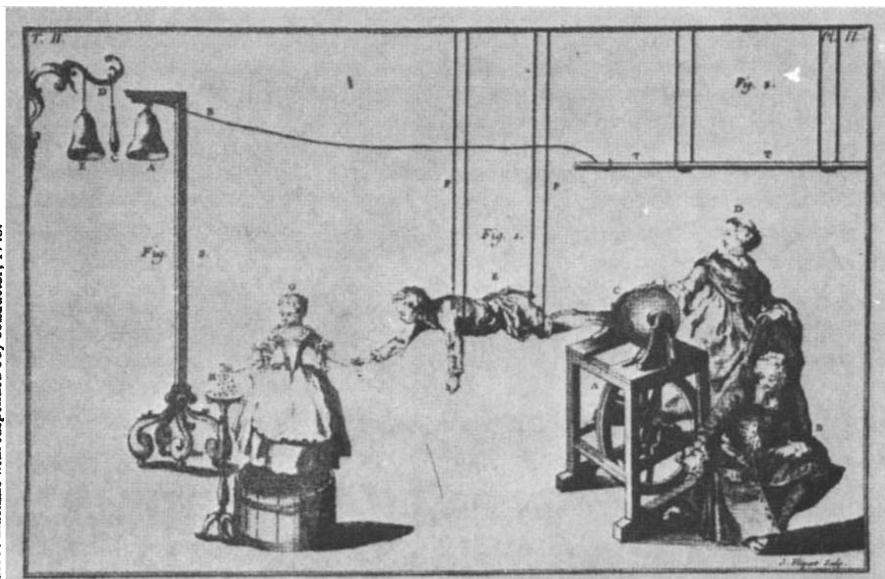
the 1750s was dominated by the followers first of Descartes, and then of Newton. The Newtonians, who by their virulent empiricism had taken Newton's 'hypotheses non fingo' much further than had been intended by their master, gained the upper hand during the 1720s. Physics took on a more restrictive and modern meaning; the subject now being restricted to astronomy and experimental physics (alternatively known by the vague English term of natural philosophy), which included mechanics, hydrostatics, pneumatics, optics, electricity and magnetism, but excluded subjects such as psychology, medicine and the biological sciences. The importance of applied mathematics in the formulation of physical laws began to be stressed, amongst others, by 's Gravesande, but this approach only gained momentum during the third and final stage. Heilbron points out that the advances made in quantifying electrical phenomena from the 1760s onwards owed nothing to the progress of mathematics. Indeed, not until the turn of the nineteenth century did electricians require mathematical techniques not fully available a hundred years earlier.

In order to make progress in electricity, required were (1) carefully worked out experimental procedures which could be replicated by different physicists, (2) accurate and reliable instruments, and (3) the isolation from the mass of experimental details of those factors that were important in producing the observed phenomena. Heilbron's narrative demonstrates how the conflicting data took many years to untangle. These three requirements are, of course, interrelated. Nature was often 'manipulated' in those areas in which new phenomena were expected. Attempts to store charge and to electrify water resulted in the discovery of the Leyden jar by von

Kleist in Germany, and Cunaeus and Van Musschenbroek in Holland, although the results were unexpected and went far beyond the experimenters' expectations. Incidentally, Heilbron honours Cunaeus with this invention, but this claim cannot be substantiated. Simultaneous and independent discoveries of this nature usually signify that the conceptual framework and experimental techniques have become available for these breakthroughs to be made.

Heilbron is fully aware of the importance of the Leyden jar. It led to the study of more powerful electrical phenomena, and it assisted with the formulation of new concepts such as that of electric circuit, intensity or level of electrification (an anticipation of potential), capacity or area of coated surface electrified, and quantity of charge, for it could readily be compared with the behaviour of liquids flowing from vessels of different sizes. The invention of the electrophorus (1775) and the parallel plate condenser (1778), based on the Franklinian explanation of the Leyden jar, not only strengthened the formulation of these concepts, but also focused on minute electrical charges (Volta's 'contact electricity'), which eventually led to the invention of the voltaic pile. Early attempts to explain this remarkable device in terms of standard electrostatic theory based on the Leyden jar failed, and it did not take long for scientists to realize that other concepts such as current intensity and electrical resistance were necessary. Thus, instruments influenced the formulation of new concepts, and the latter in turn affected instrument design. Although his conclusions are broadly in agreement with this view, Heilbron's general discussion on the development of electrostatic instruments is weak, and contains minor factual

Globe machine with suspended boy conductor, 1748.



errors.

Quite properly, Heilbron devotes an entire chapter to electrometry. He discusses the key electrometers, especially those of Robison (1770s) and Coulomb (1784). One does not, however, become aware of the existence of the great number and diversity of electrometers invented during the final decades of the eighteenth century, caused by the search for

replicability of measurements. Attempts to standardize measuring techniques and introduce electrometric standards prior to Coulomb's work failed, mainly because there was no proper understanding of what the different electrometers were actually measuring. According to Heilbron, what was lacking was the appreciation between the difference of electrostatic force at a macroscopic level (moving force or kinetic

energy), and an (idealized) microscopic force as an unobservable push or pull between elements of electrostatic charge. The triumph of Newton's theory of gravitation was to compute force by summing forces, but this distinction was not generally understood by physicists before the second half of the eighteenth century. Modern historians of science, too, have not always made this distinction when interpreting the early attempts by Bernoulli (1760) and others to relate the behaviour of electrostatic force to the inverse-square law of gravitation. Robison, Cavendish and Coulomb were the first to achieve this. They were the vanguard of a new breed of physicist, unlike their predecessors more interested in exact mathematical descriptions than in the qualitative models which had played such an important role in the early history of electricity. They formed the bridge between the qualitative work of the preceding two hundred years and the mathematical synthesis by Poisson and others in the nineteenth century. □

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Plasmid horizons

Naomi Datta

Plasmids. By Paul Broda. Pp.197. (Freeman: Oxford and San Francisco, 1979.) £6.90, \$18.50.

RESEARCH on plasmids is exciting and varied. It includes intensive study of very basic biological functions such as DNA replication. F-mediated conjugation provides an example of differentiation of structure and function in a single-celled organism, subjected to detailed genetic analysis. Insertion sequences and transposons are newly discovered and evidently important means of genetic variation allowing evolutionary changes in response to changing environment. Plasmid-determined antibiotic resistance and determinants affecting bacterial virulence are of great medical relevance, and study of the latter, in particular, may lead to new insights into bacterial pathogenicity. Plasmid synthetic and degradative enzyme systems, evolving all the time, are likely to be of increasing commercial and environmental importance. And, as well as all these reasons for the study of plasmids as they exist in nature, their use as vectors in genetic engineering opens up new horizons in biological research both academic and pragmatic. No wonder that the number of papers published on plasmids seems to be increasing exponentially. An up-to-date, comprehensive and comprehensible guide to the subject was badly needed by

undergraduates or research workers starting out in this field. Here it is — Paul Broda has provided just the thing, and it is not only newcomers to the subject who will welcome his *Plasmids*. Plasmid research has its own specializations, broadly on the lines of the subject-matter of the different chapters of this book, and people knowledgeable about any one of them cannot hope to keep up with all the others.

This is a short book, only 148 pages of text excluding references and index, and I was surprised at the amount and variety of detail packed into such a small space. The layout is organized imaginatively to assist a reader to grasp principles with which he may not be previously familiar. The many summarizing tables inserted in the text are particularly successful both as teaching aids and for quick reference. After a brief introduction to define the subject and sketch in its short history, we are presented in a thoroughly practical way with the methods used for isolating plasmids from their host cells. These methods are of two main kinds, either on a fairly large scale to obtain plasmid DNA for further study, or rapid and small-scale, adaptable to many experimental purposes. Great advances in methods of both these kinds have been made in the last few years. It is appropriate that this section should come early on in the book since much of the information on plasmids in later chapters has resulted from the use of these methods.

The chapter on the structure and evolution of plasmids illustrates how those creatures have developed by connecting as well as by diverging evolutionary paths. The use of endonucleases is described, in the characterization of plasmid DNA and also for the construction, *in vitro*, of new plasmid molecules. This implies a very broad meaning for 'evolution', but why not?

The complex subject of DNA replication is considered in Chapter 4. Plasmids are more accessible to experimental manipulation than are whole cells and therefore plasmid replication has been studied, not only for its own sake, but also to elucidate host mechanisms for, and controls over, DNA replication. This chapter covers both plasmid-determined and host functions and throws considerable light on a confusing area of research. Here, as elsewhere in the book, the limitations of current knowledge are pointed out. It is a comfort, sometimes, to be told that something is still unknown.

Transfer of DNA by conjugation in bacteria is, as far as is known, always plasmid-determined. The chapter on the subject summarizes the results of detailed investigations into the conjugative functions of F and its relations, and indicates how little is known about conjugation determined by many other, unrelated, plasmids. It is possible that the production of pili is a prerequisite for conjugation despite the instance, cited on p. 97, where they were apparently not essential. This case referred to plasmids of

group N whose pili have only recently been seen (Bradley, *Plasmid* 2, 632-636, 1979). The chapter on plasmids in human and veterinary medicine gives an overview of the astonishing evolutionary phenomenon of antibiotic resistance. Very often, within a few years of the introduction of a new antibacterial drug, bacteria resistant to that drug have appeared, the resistance being plasmid-determined. The author points out (p. 138) that the molecular events leading to the appearance of new resistance plasmids may be rare, the intensity of selection for resistance making their effects quickly evident. Gentamicin resistance, for example, is already more frequent in both Gram-negative and Gram-positive bacteria than it was at the time of publication of the papers cited on pp.114-115 of this book, a result of the widespread use of gentamicin in hospitals.

The same chapter also reviews what is known of plasmids that confer pathogenicity. There is reason to hope that research on naturally occurring plasmids, and on plasmids used as cloning vehicles for 'pathogenicity' genes, will extend our present very limited understanding of the effects of bacterial infections.

A chapter entitled "Other Plasmids" describes those determining colicinogeny, degradative functions in pseudomonas, plant tumour causation by *Agrobacterium tumefaciens*, and antibiotic production and resistance in actinomycetes. Obviously, in such a short space there must be much abbreviation and some simplification, and indeed this applies to the whole book. The references, however, are well chosen (the method of choice is explained in the preface) and numerous enough to help the reader to find out more about any of the topics covered in the text.

I have not found many errors in this book and those I have detected are fairly trivial. (To give one example, the semi-synthetic penicillins resistant to staphylococcal penicillinase are described as "cloxacillins" — they should be called isoxazoly penicillins, of which cloxacillin is one example.) Also, considering the size of the book, I have found very few omissions. The only mention of plasmids in strictly anaerobic bacteria is that referring to *Bacteroides ochraceus* (p. 139); plasmids in *Clostridium* species as well as bacteroides might have been mentioned in Chapter 6.

Two previous single-author books on plasmids are *Bacterial Plasmids* by G. G. Meynell (Macmillan, 1972) and *Infectious Multiple Drug Resistance* by S. Falkow (Pion, 1975). Both are good books but neither, in 1980, can provide for the readers who will, I believe, gratefully welcome Broda's book. □

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Towards enlightenment in radioactivity

Thaddeus J. Trenn

Radioactivity in America: Growth and Decay of a Science. By Lawrence Badash. Pp. 327. (Johns Hopkins University Press: Baltimore and London, UK, 1979.) £11.50.

THE value of history may well lie in the perspective the past can provide for the present in its relation to the future. Radioactivity is often talked around today, but generally with little understanding. Radioactive rain and snow were present long before such phenomena were discovered and investigated scientifically. Primitive man had a great fear of lightning and other forces of nature, yet with respect to radioactivity most of us still stand today as did the Neanderthals. Knowledge is power but ignorance breeds impotence. The first step to correct this source of insecurity and fear is to learn what makes it 'tick'. To this end it would be valuable to trace the historical development of man's understanding of radioactive phenomena in a manner accessible to the layman. This is what the book under review has done and why it is so important.

The science of radioactivity had a long and complex history, starting with Becquerel's discovery of natural radioactivity in 1896 and reaching to the discovery of artificial radioactivity by Joliot and Curie in 1934. A spate of major tomes, from about 1930, attests to the maturation of this multi-faceted discipline: Meyer and Schweidler, *Radioaktivität* (1927), Kohlrausch, *Radioaktivität* (1928), Rutherford, Chadwick and Ellis, *Radiations from Radioactive Substances* (1930), Curie, *Radioactivité* (1935), and regular reports on the subject which appeared until 1943 published in the *Annual Reports on the Progress of Chemistry*.

Badash narrows the subject both in time and in range by focusing upon what radioactivity research there was in America from scientists such as Boltwood, McCoy, Baskerville, Bumstead and Pegram. Rutherford and Soddy naturally play a central role not only because they were in Canada when they provided the theoretical explanation of radioactivity in 1902/3 — see *Nature* 274, 723-724; 1978 — but also because they attracted many research workers. Indeed, part of the decline of this American scientific specialty can be attributed, as Badash suggests, to the transfer of these two scientists to the other side of the Atlantic (Chapter 18).

Much of radioactivity research in America was directed towards a clarification of the genetic connections within the several radioactive decay sequences. Such radiochemical analysis fell

in between physics and traditional chemistry and could never really establish itself in America. According to Badash, this scientific specialty was also "suicidally successful" (p. 152); it was no longer viable once the genetic connections had been sorted out in uranium, thorium and actinium. The main lines of radioactivity research thus remained in Cambridge, Paris, Rome and Vienna, leading to artificial transmutation and artificial radioactivity. Even radiochemistry took on a new life as radioactivation analysis (see Broda, *Advances in Radiochemistry*, Cambridge University Press; 1950). In retrospect, we can mark the transition from radioactivity to nuclear physics and nuclear chemistry with the discovery of the neutron in 1932 followed soon after by Fermi's explanation of β -decay.

Badash does not always restrict his historical account of radioactivity to the scientific specialty which had its "growth and decay" in America. When dealing with the concept of isotopy and the group displacement laws, for example, Badash leaves the American scene for two full chapters with the centre of action in Europe. He is overtly sympathetic with Fajan's electrochemical approach, an alternative which phenomenologically did yield nearly the complete and correct formal scheme, although it was unsound (p.211) and accordingly lacked the explanatory power of Soddy's subsequent analysis, for which he alone received the Nobel Prize.

The decline of radioactivity research in America by the end of the Great War was conditioned by factors besides the loss of Rutherford to Europe, the lack of any institutional framework and the self-fulfilment of the goals of this scientific specialty. Radioactivity research "shared the same . . . poverty as other branches of science" in America (p.271). Therefore, what Badash has termed the "decay of a science" is perhaps simply the decline of a scientific sub-discipline as but one more manifestation of a general atrophy or perhaps the "immaturity" (p.274) of American science needing the fertilizing immigration of the 1930s to develop.

The reader may consider much of this somewhat remote from the current pressing concerns about nuclear reactors and atomic bombs. Nevertheless, all this began with radioactivity; the emission of radioactive radiations and the self-transmutation of one isotope to another being nuclear processes. That nuclei can also fission into larger fractions, and that under certain conditions this fission process can lead to a chain reaction are also natural phenomena that need to be better understood. Even scientists were surprised to learn recently that our technological 'eighth wonder' — the controlled fission reactor — had been anticipated by nature when dinosaurs still roamed the earth. Inherent checks and balances prevented this natural reactor from exploding like an

atomic bomb, since the critical state, once reached, tends to turn itself off. The very heat generated by the onset of an uncontrolled chain reaction tends to liquefy the dense and compact solid substance dispersing it, perforce eliminating the concentrated condition so essential for the onset and continuity of the critical state within a mass of fissile material.

Only if a chain reaction can be achieved and executed nearly instantaneously by intentionally overcoming the barriers nature has set can the 'atomic-bomb' effect be realized. The concentration of fissionable material in a nuclear reactor is not appropriate for this effect. In so-called thermal reactors using the isotope U-235, fissile to *slow* neutrons (neutrons of relatively low energy which have been 'moderated' to 'thermal' velocities), the chain reaction of the nearly instantaneous type required for the 'atomic-bomb' effect simply cannot occur. For the same reason, an atomic bomb consisting solely of the rare isotope U-235 would only fizzle. It is

natural uranium, U-238, which provides the main contribution to the 'atomic-bomb' effect, since it can be fissioned with neutrons which are *fast* enough to yield a chain reaction having the required celerity. Once these and other consequences of nature's inherent control mechanisms become better known, there will be less public obsession with the alleged dangerous proliferation in the supply of U-235 and allied (*slow*-neutron) fissile substances like plutonium.

Yet, to have expected the present work to deal with the totality of our misunderstanding about radioactivity and its nuclear progeny would be unreasonable. The scales can be removed from the public perception only in gradual stages. That Badash has taken us a significant step forward is perhaps a sufficient achievement.

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Astronomical oddities

Paul Murdin

Monsters in the Sky. By Paolo Maffei. Translated by Mirella and Riccardo Giacconi. Pp.241. (MIT: Cambridge, Massachusetts, and London, UK, 1980.) \$15.

THE monsters in the title of this book are not constellation figures, inhabitants of UFOs, dragons or other overtly mythical creatures. Monsters is used in the same sense as in the section of the *Thesaurus* called Unconformity, where it is associated with wonder, curiosity, missing link and queer fish. The "monsters in the sky" are thus unusual astronomical objects. They are the objects, says Paolo Maffei, which were better left out of his previous book, *Beyond the Moon*, on the more straightforward parts of astronomy, because they were so unusual that they were confusing. The present book assumes the elementary knowledge of astronomy contained in the previous one and builds on it to give accounts of black holes, quasars and active galaxies, the missing mass, novae and supernovae, Vulcan and transplutonian planets, and comets (a little surprisingly this, since comets are not as unusual as all that). Each chapter is written in a fair amount of detail, with the stages laid out logically by which astronomers came to their occasionally monstrous conclusions. The chapter on Eta Carinae is the most detailed in this respect, starting with its historical light curve, laying out just how Eta Carinae differs from other superficially similar kinds of stars,

discussing its infrared properties and Gratton's model of the star and concluding with its possible link with the Hubble-Sandage Variables.

The book would be enjoyed by an amateur astronomer who is ready to get to grips with some detail in astronomy. The accounts of the history of the subjects with which it deals make it a book which, say, graduate students would find it profitable to read, so that their thesis on the subjects in which they may be researching might then reference more than the last couple of years' *Astrophysical Journal*. The bibliography to each chapter gives enough references, typically to review articles of the kind that appear in *Nature*, for an easy entry into the literature dating before 1976 when the first edition of the book appeared. This, incidentally, is a translation of the second Italian edition (1979). I can pay the translation the highest compliment of all, that if it was not for a few footnotes accredited to the translators, one would scarcely notice that the book was translated.

Maffei's style is engagingly informal, although always scientific. The effects of close encounters of an astronaut with the tidal forces and event horizon of a black hole, for instance, are described with enough realism to make the image specific but not overplayed in the repellently sensational, even mystical, way which characterizes many journalistic accounts. On the contrary, this is a book which is written by an astronomer who knows the astronomical literature and has considerable skill in interpreting it at a popular level. □

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The AI saga

Donald Michie

Machines Who Think. By P. McCorduck. Pp.375. (Freeman: Oxford, 1979.) £7.80.

THE task of the scientific referee is a well known contradiction. If he is himself a part of that particular action, then he cannot be impartial. If he is not, then how can he be technically competent in his task? The writing of scientific history is in similar case. It is therefore the more remarkable that Pamela McCorduck's gossipy saga on artificial intelligence, aspiring neither to impartiality nor to technical competence, has scored an undoubted overall success.

Her partiality is to the American part of the story, especially to Pittsburgh. Her saving gift is vitality, which carries her eagerly into previously unexplored catacombs of AI's pre-history. Using gold as the construction material the lame Greek god Hephaistos fashioned female attendants to help him walk. These robots not only walked, but also gave utterance from the promptings of inbuilt intelligence. A bronze android named Talos, Zeus' love-gift to Europa, patrolled the shores of Crete three times a day and hurled rocks at invaders. On catching any, he would heat himself up and fry the victim in a fiery embrace. Pandora, another female android from Hephaistos' shop, was developed under contract from Zeus to punish mankind for accepting Prometheus' gift of fire.

Joseph Golem was the artefact of the 16th century Loew, chief rabbi of Prague and friend of Johannes Kepler. The Golem's use was to spy on Gentiles. In reporting on his missions he was somewhat hampered by being mute.

Mary Shelley's tale is brought hauntingly to life, with Dr Frankenstein's creation driven half-mad with loneliness and his maker's evident physical repugnance for him. "Misery makes me a fiend!" says the 8-ft monster, and begs his master to fashion a soul-mate for him. Frankenstein, having promised this, abandons the task, with devastating effect on the creature's subsequent behaviour.

McCorduck's shrewdest move is to have gathered lengthy oral histories from numerous tape-recorded interviews for later interweaving with pre-existing records. It is startling to read Marvin Minsky's words of 1956:

The important result that would be looked for would be that the machine would tend to build up within itself an abstract model of the environment in which it is placed. If it were given a problem, it could first explore solutions within the internal abstract model of the environment and then attempt external experiments. Because of this preliminary internal study, these external experiments would appear to be rather clever, and the behaviour would have to be regarded as rather 'imaginative'.

The subsequent role of heuristically guided processes of search and inference performed on internally stored world-models has been central to AI. So too has the development of special forms of computer language in which to express what the machine knows about its world. It is therefore illuminating to read John McCarthy, saying at this same formative Dartmouth Conference that it was desirable "to attempt to construct an artificial language which a computer can be programmed to use on problems requiring conjecture and self-reference . . .".

Occasional flashes of this kind give welcome relief from the human-interest magazine style. But the latter also has its moments, not least when McCorduck permits herself to write directly from her own resources of wit and perception. On intellectual fashion:

No sooner do hemlines go down with

enormous fanfare than they go up again, the provinces growing dizzy with trying to keep pace . . . MIT thinks itself stylish, but outsiders have been known to call it faddish. Carnegie-Mellon, on the contrary, represents old-world craftsmanship, attending to detail and using the finest materials . . . But classic can be stodgy: if Queen Elizabeth of England bought artificial intelligence, she'd surely buy at Carnegie-Mellon.

For those not already members of the AI subculture, the parade of personalities, their doings and sayings, is liable to pall. Chapter after chapter is devoted to attacks by American polemicists of whom the general scientific world has never heard, aimed at targets whose names are scarcely better known, concerning philosophical issues about which few readers will excite themselves one way or the other. Yet during the same period a world-renowned British physical scientist published a concretely phrased indictment of robotics-

oriented AI activity in Britain, and this led to its almost total dismantling and dispersal. McCorduck passes over the consequential event with a brief mention: hardly excusable in a serious history.

But McCorduck's book is not offered as a serious history, any more than Aubrey's *Brief Lives* is a serious history of Oxford, or David Niven's *Bring on the Empty Horses* is a serious history of Hollywood. The reader should not come to Pamela McCorduck's whimsically titled book for instruction, or for any deep insight. But by stretching his limbs for a while in her brightly peopled landscape he can assuredly acquire, along with much enjoyment, a feel for the adventures, and the adventurers, of a scientific story at its beginning. □

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Natural illustrations

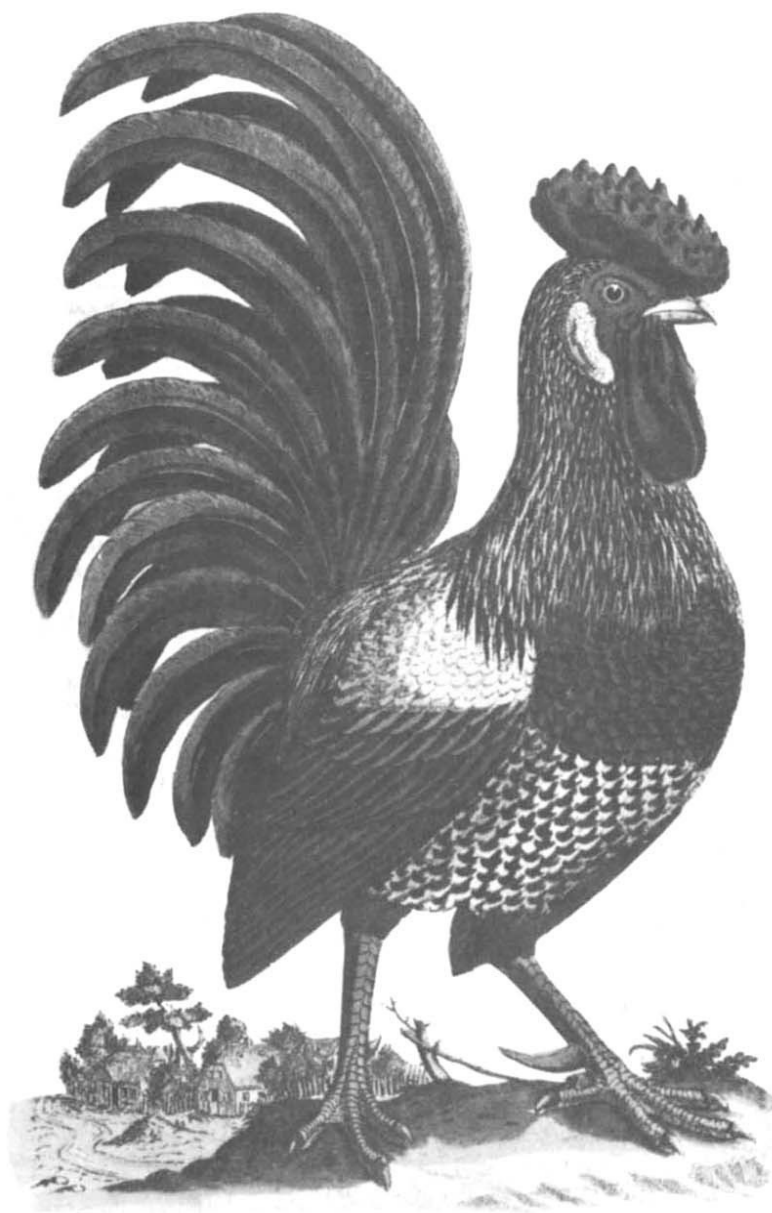
R.D. Meikle

Nature into Art. By H. Buchanan. Pp.220. (Weidenfeld and Nicolson: London, UK, 1979.) £15.

THE title neatly indicates the scope and object of this handsome volume. It is a concise survey of natural history books with fine, coloured illustrations, for the most part published between 1700 and the middle of the succeeding century. The accompanying text is concerned not so much with the scientific importance of the publications or the scientific exactitude of the plates as with their artistic merit and technical excellence.

To deal with such a vast subject within the compass of 220 pages means, of course, that even the finest illustrators are represented by only a very few examples of their work, while many of the less distinguished must be passed by, sometimes without as much as honourable mention. Selections seldom please everyone, but few are better qualified than Handasyde Buchanan, with forty years' experience of such splendid literature, to judge what must be included, and to decide — a more invidious task — what may be omitted. His choice is well balanced, without being too detached, just as his text is concise without becoming a bare catalogue of facts. The notes on printing techniques will be particularly welcome to those who have puzzled over *del.*, *dir.*, *sculp. imp.* and *pinx.*, or who have failed to grasp the distinctions between copper and stipple engravings, mezzotints and aquatints. An appendix of book prices

Plate from Johann Leonhard Frisch's *Vorstellung der Vögel in Deutschland*, Berlin 1733-63.



current in 1934 seems, at first sight, to be a catalogue of missed opportunities, until one recalls that, in the same year, a professional naturalist, lucky enough to find work, was considered affluent on a salary of £400 p.a.

Illustrations are essential to the botanist or zoologist, indeed it may be conceded that one good plate is worth a dozen paragraphs of descriptive text, however carefully penned. But it may be questioned if more than a very few of these "grand" works were intended for scientific study. Most were destined for the libraries of those enlightened sections of the gentry who, drawn to the study of plants and animals by the lucid codifications of Linnaeus, sought, in botany and zoology, a pastime less boisterous and bloody than the chase. With few exceptions the text which accompanies such glorious illustration is a sad let-down, even that of the monumental *Flora Graeca*, too often revealing an absence of accurate data, while the *Botanical Magazine* belied its name for a hundred years by serving up what often amounted to little more than the shadow of

a commentary.

The relative immobility of plants, and their clear, definite outlines, suited the processes of engraving; indeed, in the hands of skilled engravers, the very limitations of the technique were salutary, and, with the rise of lithography, plant illustration (always excepting the work of W.H. Fitch) too often degenerated into the blurred prettiness of *The Ladies' Flower Garden*. On the other hand, lithography might almost be said to have been made for the softer world of fur and feather. Set against the splendours of Lear and Gould, many earlier illustrations of mammals (and to a lesser degree, birds) have a quaint, almost comical look, even in books like Catesby's *Natural History of Carolina*, where the plant illustrations are perfectly acceptable. Ignorance of animal movement and anatomy was, no doubt, a factor also contributing to the comparatively late development of good animal illustration: the lifeless butterflies which Ehret and his contemporaries scattered so liberally over their botanical plates are, to my mind, an unwelcome adornment, detracting from,

rather than adding to, the beauty of the flowers.

So many aspects of *Nature into Art* are pleasing that it seems almost ungrateful to end on a sour note, but I doubt if anyone will commend the extraordinary system whereby groups of legends are referred backwards or forwards to invisible page numbers, a scheme which might have been designed to ensure maximum exasperation and minimum accuracy. Less irritating, because it may have been unavoidable, is the frequent spread of animal plates across facing pages, with a hinge down the middle, breaking the poor creature's spine, and sadly detracting from the artistic effect. These are, however, small criticisms of a book which will delight all those who value a tradition (fortunately still very much alive) which reconciles, in the happiest possible way, the several objectives of art and science. □

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Objectives for technology

Steven L. Del Sesto

The History and Philosophy of Technology. Edited by G. Bugliarello and D.B. Doner. Pp. 384. (University of Illinois Press: Urbana, Chicago and London, UK, 1979.) £12.25.

THIS book attempts to locate the problems of contemporary technology within the mainstream of philosophical and historical inquiry. It is the belated publication of a collection of papers first given at the International Symposium on the History and Philosophy of Technology, May 14-16, 1973, at the University of Illinois, Chicago Circle. The event was sponsored jointly by the Colleges of Engineering and Liberal Arts, attesting to the interdisciplinary character of the book and its principal subject matter. The book therefore reflects the essential beliefs of the members of the symposium: that technology plays an increasingly centralized role in our societies and our lives, and that we might study it systematically by way of the substantive and methodological approaches offered by history and philosophy.

To achieve this task, the book consists of some two dozen essays, structured around three sub-headings. The first, the history of technology, contains a number of useful papers clarifying the connections between history, society and technology. Among the papers in this section is an original piece by A.L. Donovan concerning the relationships between science and technological innovation, described by way of an

account of the close personal association between Joseph Black and James Watt in the invention of the separate condenser. There is also an insightful paper by N. Rosenberg discussing the complex interactions between technology, society, economy and values; he argues that we cannot predict the consequences of technological change without understanding the values of a society. And finally, some methodological suggestions for studying the relationships between science and technology are advanced by D.S.L. Cardwell and C.S. Smith. Smith posits that the ultimate understanding of invention and discovery depends more on the study of material objects than on the written records which historians generally use, while Cardwell argues that much of physical science as we know it was shaped by man's early industrial experiences, and not simply the other way around as many people believe.

The papers in the second section are grouped under the heading, the philosophy of technology. D. Wojick, for example, shows the utility of a Kuhnian model of scientific change to describe what he calls "technological revolutions" (nuclear power, food additives, and water resources management in the United States are his examples); while M. Bunge gets down to the serious job of characterizing the relationships between technology and the various branches of philosophy: epistemology, metaphysics, axiology, ethics and legal philosophy. In addition, there are papers concerning the relation between modern technology and political theory, as well as essays which trace the history of the philosophy of technology to its early origins. Many papers in this

section contribute detailed bibliographical materials that will be quite useful to specialists and philosophers, but are probably less interesting to the general reader.

The book concludes with a section on the future of technology which I found too brief and largely disappointing, perhaps because of the many new developments since the symposium was first held more than six years ago. I would like to have seen the important relationships between technology and social change explored more directly, and there could have been additional material regarding the anticipated affects of contemporary technological enterprises such as electronic message transfer, new biological techniques, computers, and so on. Furthermore, little attention was afforded either to the important issue of technology assessment, or to the areas of alternative technology and technology transfer. What we get are eloquently written but largely familiar statements like "technology subverts human values and impinges upon individual freedom", "we must understand technology to help provide for the continued existence of the human race", and the like. No-one denies the importance of such issues, but serious scholars of technology are apt to find the section on the future of technology less useful than the preceding ones; it is at best incomplete.

Beyond the content and organization of the individual essays, the book as a whole stresses at least two general themes, which seem to have been the central messages of the symposium. First, it is clear that the phenomenon of technology deserves continued, if not more systematic, study by scholars. Comprehensive studies and

programmes concerned with technology/society relationships should receive continued support. But the essential task, as M. Kranzberg notes in his introductory essay, is not simply study and reflection, but to embark on the difficult job of developing more precise conceptual, theoretical and methodological frameworks that allow the codification of diverse results from many fields of inquiry. Specifically, I would add that we need to work on higher level theories of social and technological change which can better explain how we got where we are, and where we might be headed in the future. This means, among other things, reaching basic conceptual agreement on key terms often used loosely and broadly — terms like “rationality”, “automation”, “efficiency”, “social change”, “science”, and the very concept of “technology” itself. Many of the essays in the book outline ways we may begin to tackle these important tasks.

Second, and I believe quite correctly, several papers put forth the theme that society must develop better moral and ethical perspectives on the control and improvement of technology, and that scholars of technology have a key role in the effort to produce better and more humane technologies in the years ahead. The job cannot be done simply by politicians, technocrats and administrators, but must be informed by the arts, humanities and the social and natural sciences as well. The task is truly interdisciplinary in character; and greater levels of improvement will not emerge without broadly based action which ranges far beyond the efforts of historians and philosophers working alone.

In sum, this compilation of essays will be useful to specialists as well as to general readers wishing to grasp the broad contours of the study of technology. It is well documented and mostly clearly written, although the lack of a subject index is annoying and hampers its value as a research tool; this, I believe, should be a major function of such a book. Nevertheless, I think anyone seeking an understanding of the full range of perspectives on technology and society will find time invested here worthwhile. The book's essays illustrate well how technology affects, both positively and adversely, social and political institutions, and how the human psyche — those ways in which we think, act, and relate to each other and the world — are influenced in directions that we are only beginning to foresee. It is to be hoped that such historical and philosophical materials can inform policy objectives, and help us decide on future technological choices as individuals and societies. □

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Understanding non-crystalline materials

D.C. Licciardello

Electronic Processes in Non-crystalline Materials. Second edition. By N.F. Mott and E.A. Davis. Pp.590. (Clarendon/Oxford University Press: Oxford, 1979.) £29.

THE second edition of this enormously successful book is faithful to the Mott and Davis style. The authors expand on their laudable attempt at unifying the vastly growing experimental literature in this field within the framework of some very general theoretical concepts. The book represents the single work which provides a focus to a field which, before Mott and Davis, had a divergent breadth.

The task is particularly monumental for at least two reasons. Firstly, the theoretical foundation on which the treatise is based is not well established. Indeed, the current literature is rife with new calculations which are at variance with some of the more fundamental ideas on which the exposition depends heavily, such as the minimum metallic conductivity. Secondly, the experiments being reported give results which still vary, to some extent, from laboratory to laboratory. This is partly due to the substandard quality of early experimental work, since only recently has enough attention been brought to the subject (largely through this book) to attract the interest of the world's best solid state physicists. Mostly, however, non-crystalline materials add a new dimension to the experimenter's problem in that materials must be characterized by method of preparation in addition to chemical makeup, e.g. one does not describe the generic properties of amorphous silicon.

The book describes the basic theory in the first four chapters. Care is taken, in traditional solid state format, to separate the non-interacting problem (Chapters 1–3) from the interacting one (Chapter 4). The basic theoretical problem, in the first case, concerns the behaviour of an electron in a random potential. The theory was first illuminated by P.W. Anderson in 1958, and Mott and Davis consider the Anderson mechanism for electron localization fundamental to the understanding of the properties of non-crystalline systems. The theory is discussed in some detail within the context of application to real systems. Thus, the exposition is much less formal than the original Anderson treatise and attempts are made to recast the concepts within a descriptive band theory. Herein lies the real value of this work for the solid state experimentalist.

The important problem of conduction in random media is discussed at length. The book demonstrates within fairly simple

analysis the absence of conductivity for Anderson localized electrons. The transition to conducting behaviour when the randomness is reduced or the electron density is increased is treated and Mott's notion of the minimum metallic conductivity is described.

The effect of correlation on transport and localization (Mott transition) persists and is expanded upon in the second edition. The term Fermi glass is used to denote a system of localized electrons in which localization may be due to randomness (Anderson) or electronic correlation (Mott). Mott and Davis discuss, as a class, those systems which undergo transitions from the Fermi glass state to metallic behaviour. To be sure, the two phenomena are distinguished, although the extent to which they are fundamentally different is again a current theoretical research topic. The discussion (Chapter 4) on metal-insulator transitions is unusual for the text in that the canonical examples of these effects are crystalline. Thus, Mott and Davis discuss doped semiconductors and narrow band transition metals.

The book, however, is mainly concerned with amorphous semiconductors. These are divided somewhat arbitrarily according to the local valence requirements of their constituents. Selenium and tellurium, however, are treated separately from the compound chalcogenide glasses. Mott and Davis make an invaluable contribution by extracting the most relevant details of the vast literature on these materials and by describing the basic electronic measurements in comparison. This kind of contribution defines the field and makes it particularly amenable to the newcomer.

Any treatise which ventures into a synthesis of a rapidly advancing subject runs the risk of being dated fairly quickly. The problem is even more serious when the authors engage, as is done here, in an explication of many measurements in terms of a general theoretical framework which, as pointed out above, is still controversial. For example, it is fair to say that at this writing, current renormalization group calculations give a conductivity at the Anderson transition with no minimum value. Much is made, in the second edition, of experiments on conduction in an inversion layer at the semiconductor/glass interface. The authors use this to support the notion of a universal minimum metallic conductivity in two dimensions. Again, there are recent calculations which suggest the absence of diffusion altogether in two dimensions. More recent experiments (not included in the book) tend to support this, although the mechanism for localization is still unclear.

The basic model presented for understanding non-crystalline semiconductors is a picture of an ideal structure into which the authors imbed a certain concentration of “defects”. The concept of a defect

within a random structure is difficult to define; however, the idea is that the electronic properties of the ideal structure would be similar to that of the corresponding crystal (if one exists). Thus, the defect, in this sense, is important in determining the specific electronic properties of the amorphous semiconductor. This is a rather extreme point of view for the structure of glass, in my opinion, yet the authors are able to cast much of the data within this framework. It is fair to point out, however, that models do exist which minimize the effect of local

structural inhomogeneities but do enjoy similar success in explaining much of the data. These theories invoke a strong lattice interaction and, indeed, Mott and Davis discuss the simple negative-U model first introduced by Anderson but narrowly interpreted here. Thus, the basic electronic structure for glassy semiconductors is not universally agreed.

The reviewer with the broad view may dismiss much of the current controversy as detail and indeed it is somewhat inconsequential when the contribution is taken as a whole. It is certainly true that some of the

analysis given in this book will be finally shown to be incorrect. This is the risk the authors boldly took in attempting to characterize and define a new branch of solid state physics. It is fitting that the Nobel Prize in physics was awarded to Sir Nevill Mott for contributions to this field, many of which are included in this book. □

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Heavy stone circles

R.J.C. Atkinson

Rings of Stone: The Prehistoric Stone Circles of Britain and Ireland. By Aubrey Burl, with photographs by Edward Piper. Pp.280. (Frances Lincoln/Weidenfeld and Nicolson: London, UK, 1979.) £9.95.

AUBREY BURL'S position as the leading authority on stone circles was established by the publication of his *Stone Circles of the British Isles* (Yale University Press, 1976), a work for scholars which is not likely to be superseded within the present century. *Rings of Stone* is a companion volume for the layman, extensively and admirably illustrated, but unencumbered by the *apparatus criticus* of scholarship. There is a glossary, a reading-list confined to books and monographs, and an index. The reader who is stimulated to seek primary sources must consult the bibliography and gazetteer of his earlier book.

Apart from its weight (1.2 kg) and its 27 colour plates on art paper, which are at the mercy of the fine soft rain which falls so

frequently on stone circles, this is an excellent vade-mecum for the holiday traveller with a purpose, whether it be specifically archaeological or aesthetic. The amateur prehistorian will find it useful in guiding him to the best-preserved and most easily visited sites. The traveller bent, in the Romantic tradition, upon viewing these rugged and enigmatic works of man against their landscape background will be well served also, provided that he remembers that the rocky and treeless terrains which now surround many stone circles are themselves the product of man (and indeed of the builders of the circles themselves), and not of nature alone.

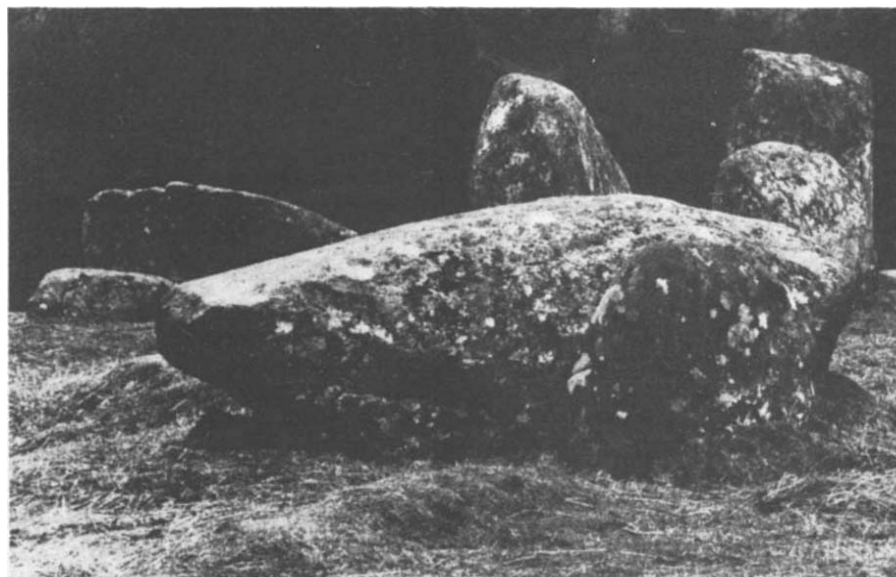
Readers with either purpose will be guided in their travels by the latter two-thirds of this book, which describes and illustrates fifty stone circles in Britain and Ireland which are well preserved and easily visited. For each region there is a map showing the sites and the main roads. For each site there is a map reference and a plan and photographs, as well as a textual description which will delight the reader by its aptness and sensitivity, either from his prior knowledge of the monument or after his first visit. It is a pity, however, that there are no figured scales for the plans, which vary widely in their ratio of

reproduction; but the principal dimensions are given in the headings.

The photographs, of which there are 106 in black-and-white as well as those in colour, are mainly the work of Edward Piper, who has inherited much of the genius of his father, the artist John Piper. Apart from one or two of the colour plates, in which the printers have achieved an overall hue which is displeasingly chlorotic, this is the finest set of pictures of prehistoric antiquities which has ever been assembled between one pair of covers. It is a picture-book in its own right, quite apart from the accompanying text, and it will stimulate the reader to see more of these sites than the photographs can show. For those who are unable, or unwilling, to venture far from the slippered comfort of their own firesides, the combination of visual and verbal description conveys very well the variety of size and character of stone circles, and readily explains their appeal to the antiquaries and topographers of the Romantic Age, which is still happily not dead.

This guide for the visitor is preceded by ten short chapters, in which Burl summarizes his unrivalled knowledge of the stone circles of the British Isles and gives his own views about their purpose and date. He begins with one of the principal regional groups, which is also morphologically and archaeologically the most coherent: the Recumbent Stone Circles of north-eastern Scotland. Here ten or eleven stones, rising in height towards a pair flanking a recumbent slab lying on edge and often with a top surface accurately horizontal, surround a low annular cairn of small boulders with a small central open space, in which excavation has frequently revealed signs of burning and small deposits of cremated human bone, accompanied significantly often by fragments of quartz.

The paucity of the human remains leads Burl to conclude, and doubtless rightly, that these were not cemeteries, but places of worship for a single extended family, sanctified at their inception by a token burial or burials, perhaps as the sequel to deliberate sacrifice. He notes, however, that although a single family could have



Edward Piper.

raised all the upright stones, the transport of the recumbent stone (typically of 20 tonnes or more) required the combined efforts of the able-bodied members (and perhaps the males only) of a number of families. This provides a tantalizing glimpse of community action, and perhaps of a clan organization, in the late third and early second millennium BC; but its details are beyond the limits of valid inference from the evidence.

Burl also shows, in more detail than in his earlier book, that the position of almost all the recumbent stones lies within the azimuth range of the moon at its minimum declination; and he therefore posits a symbolic link between the moon and the dead who sanctified these sites.

In the remaining hundreds of stone circles elsewhere there is little to be seen of any recurrent pattern, save that they are of stone and approximately circular. Some but not all of the few which have been comprehensively excavated have produced burials, but again of a token or dedicatory kind, or as manifestly later additions, in the Early Bronze Age, to monuments built centuries before. They vary greatly in size,

from the huge ring at Avebury to tiny circles which would very nearly fit into the study in which I write this review; and in date they seem to cover a period of about two thousand years, starting around 3,500 BC. Within such wide ranges it is probably futile, as Burl implicitly recognizes, to seek any simple or universal explanation. It is better to admit, however reluctantly, that there are many interesting questions about the prehistoric past which are *per se* unanswerable, simply because there are no documents to give a clue to motives. Motive, except in the trivial sense of intending to do what was done, cannot be inferred from inanimate objects and structures. In answer to the question 'Why?' the evidence is dumb.

This otherwise excellent book dismisses too forthrightly the putative evidence from stone circles for the early development of scientific concepts. For Burl, the megalithic yard is "a chimaera, a grotesque statistical misconception". It is indeed a product of statistical inference; but two independent treatments of the data by different means have agreed, at the least, that further investigation is necessary

(*Trans. R. Soc. Lond. A*276, 1974; *J. R. stat. Soc. A*139, 1976).

Burl likewise implicitly rejects Thom's hypotheses about how the non-circular rings (flattened circles, eggs, compound rings) were set out, on the grounds that alternative constructions, using only pegs and a loop of measuring cord, yield an equally good fit to the stones surveyed. He may be right; but he ignores or evades the point that even if these shapes, manifestly deliberate and not accidental, are no more than the fossilized remains of games with pegs and string, they none the less constitute the foundations, gropingly laid maybe, of pure mathematics.

The astronomical aspects of stone circles can be assessed only statistically, using the largest samples available, because putatively significant alignments can obviously occur by chance. Independent checks of the claims of Thom and his followers are only now in progress. Their outcome should not be prematurely judged, in advance of publication. □

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Electronic theory of cancer

M. M. Coombs

Submolecular Biology and Cancer. Ciba Foundation Symposium, No. 67. Pp. 349. (Excerpta Medica: Amsterdam, 1979.) \$42.50, Dfl. 87.

THIS book reports the proceedings of the 67th Ciba Foundation symposium, held in honour of Albert Szent-Györgyi on the occasion of his 85th birthday in 1978.

In an introduction by the Chairman (R. J. P. Williams) it is noted that although "submolecular" refers to systems at a lower level than whole molecules, in the present context it is largely confined to electrons. Szent-Györgyi next outlines his views on "the living state and cancer", and the possible relationships between submolecular biology and cancer are enumerated. The greater part of the book is devoted to fifteen lectures, each with its own discussion, and ends with a general consideration of Szent-Györgyi's hypothesis.

The sharp distinction between living and inanimate matter leads Szent-Györgyi to conclude that a specific physical state underlies life. The subtle reactivity of living systems is thought of as an inherent property of the constituent proteins, brought about by removal of single electrons from their otherwise fully occupied orbitals. Positive electron holes

thus produced lead to electrical conductivity, with the ability of the desaturated proteins to carry information in the form of electrical signals. It is proposed that "the living state is the electronically desaturated state of molecules, and the degree of development and differentiation is a function of the degree of electronic desaturation". When life began on Earth the atmosphere lacked molecular oxygen; electronic desaturation of proteins was accomplished by simple aldehydes, such as methylglyoxal ($\text{CH}_3\text{CO}\cdot\text{CHO}$), which are good electron acceptors. This led to a low degree of development, termed the α -state. Later the advent of atmospheric oxygen, a much better electron acceptor, led to the highly developed β -state existing today. Here a related molecule, ascorbic acid (vitamin C), acts essentially as a catalyst for the electronic desaturation of proteins by oxygen. Cancer is thought of as a reversion to the α -state.

It is obvious from the studies described in the subsequent chapters that little evidence can be found for electrical conductivity in proteins treated with electron acceptors, although here the virtual impossibility of making measurements on proteins in their native state may obscure true results. Nor is the anti-cancer activity of certain aldehydes apparently mediated in the way suggested by the hypothesis; moreover, it now seems very doubtful whether vitamin C is useful in the clinical treatment of the disease. Furthermore, experiment does not support the idea that there are differences in free radical levels between normal and tumour

IMAGE
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REASONS

Albert Szent-Györgyi.

cells. However, R. Damadian, in developing the technique of whole-body nuclear magnetic resonance scanning, can distinguish normal from cancer tissue by the prolonged relaxation of protons in the latter. This indicates increased disorder in the structure of water in tumours, but just how this ties in with Szent-Györgyi's theory is unclear. To summarize, it is disappointing that this novel hypothesis has so far failed to find solid experimental confirmation.

The book is well produced and free from errors; its cost does not seem unreasonable in this age of unreasonable prices. □

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Courtesy of Owen & Moroney, London.

Fludd's philosophy

P. M. Rattansi

Robert Fludd and His Philosophical Key. Introduction by Allen G. Debus. Pp.156. (Neale Watson: New York, 1979.) \$40, £20.

In 1620 Francis Bacon published his *Novum Organum*, with a dedication to James I, urging him to support his enterprise, "that so at length, after the lapse of so many ages, philosophy and the sciences may no longer float in air, but rest on the solid foundation of experience of every kind . . .". At about the same time, the physician Robert Fludd had completed a *Philosophical Key* "to unlock and open the meanings of that Macrocosmicall and Microcosmicall Philosophy". He, too, dedicated it to James I, hoping the King's patronage would shield him "from the Harpys talents and envious endeavours of this World".

Fludd agreed with Bacon that Aristotelian science was to be replaced by one based on "ocular demonstration" and dedicated to the relief of human misery. Like him, he praised atomism, but was more inclined to explain nature's operations in terms of a 'spirit' active in all things. But Fludd believed in a 'Christian' philosophy of nature, the principles of which would be drawn from the account of creation in the text of Genesis. Bacon condemned the "unwholesome mixture of things human and divine" since it gave rise to "not only a fantastic philosophy but also an heretical religion". Fludd associated the greatest secret in nature, the principle of life, with light and the breath of the Lord, while Bacon denounced such notions as "superstitious".

His major works had already appeared in Latin, and had involved Fludd in controversies with Kepler, Mersenne and Gassendi. The *Key* was written in English to silence critics at home. Fludd thought his "experimental demonstrations" would convince readers of the truth of his ideas. Relying on an analogy between the Sun in the Universe, the heart in the human body, gold in the metalline and wheat in the vegetable kingdoms, Fludd chose to concentrate on experiments on wheat. After all, had it not been compared to the Second Person of the Trinity, and was it not Christ's referring to bread as his body which had set the Christian world "in combustion"?

The planting of the seed in the ground and the generation of wheat had exemplified the pattern of death and resurrection to the Christian alchemist. Fludd pointed out that as the plant grew, the quintessence in it rose to the stalk and was concentrated in the wheat grain. By a sympathetic magnetism, the most spiritual part of the plant would then draw down

more and more of the aerial spirit. This process could be emulated by the chemist, and Fludd believed he had succeeded in obtaining the universal spirit in the form of a "white christalline" substance.

Fludd's claims may seem extravagant to us, his reasoning contorted and his "ocular demonstrations" quite unconvincing. It is discomforting, therefore, to recognize in his work themes which recent historical research has unearthed in the alchemical studies which occupied a great deal of Isaac Newton's most creative period: the true interpretation of the alchemical foundation text, the 'Emerald Tablet'; the 'magnet' to draw down the aerial quintessence; and the involvement of light in generation in the three kingdoms of

nature. He represents a style of thought which cannot be neglected if we are fully to comprehend those whom we recognize as the pioneers of modern science.

The *Key* has been published for the first time from the manuscript at Trinity College, Cambridge, with an Introduction by Allen G. Debus. The text itself occupies 93 pages and has been supplemented with an extensive account of the life and work of Fludd. The work, written in a rhapsodic style which occasionally breaks into verse, makes Fludd's ideas a little more accessible to the modern reader. □

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Recombination in phage and fungi

H.L.K. Whitehouse

Genetic Recombination: Thinking about it in Phage and Fungi. By F. W. Stahl. Pp.333. (Freeman: San Francisco and Oxford, 1979.) £12.90.

THE publication of a book on genetic recombination by one of the leading workers in the field is an important event for geneticists. Recombination was discovered in the early years of this century and became a central issue in genetics with the realization that exchange of segments between homologous chromosomes was involved, in addition to the random orientation of the paternal and maternal members of each chromosome pair on the spindle at the first meiotic division. Recombination took on a new dimension about 30 years ago with the discovery of its occurrence in prokaryotes, and with the recognition that both in prokaryotes and eukaryotes understanding the process of exchange was likely to require detailed knowledge of the biochemistry of DNA. Since that time it has been essential, if a general view of the subject was to be obtained, that results derived from prokaryotes and eukaryotes should be integrated. To achieve such integration is difficult, as the life-cycles, and therefore the experimental techniques and kind of information obtained, are so different: the prokaryote- and the eukaryote-worker speak such different languages that they may barely understand one another. It is greatly to Stahl's credit that he has attempted in this book to bring about a synthesis of results from bacteriophages and from fungi, as these are the organisms that have contributed most from each kingdom. The phages discussed are lambda and T4 of *Escherichia coli*.

The first two chapters give the basic information about the exchange process obtained from progeny chosen on a random basis, in fungi and phages, respectively; and the next two chapters give the fuller picture when both products of an exchange are recovered — that is, tetrad analysis and single burst analysis. The implications of the duplex structure of DNA are introduced in Chapter 5. Then follows an important chapter on the surprising results that have been obtained, both in phages and fungi, from investigations with very closely linked mutants. Together with an appendix giving the algebraic basis of the concepts introduced, this chapter constitutes one quarter of the entire book. The following two are concerned with conversion and its association with crossing-over; and, after a consideration of recombination chemistry, the book concludes with lengthy discussions of current hypotheses. There is no account of transposons or phage Mu.

The lay-out of the chapters, although logical, does not necessarily make the subject matter easy to comprehend, and certainly leads to considerable repetition. Thus, having learnt in the first chapter that crossing-over takes place between homologous chromosomes, one has in a later chapter to replace this with the notion that the exchanges occur between daughter chromatids rather than whole chromosomes. Again, in discussing aberrant asci, one of the five basic kinds is treated in a different chapter from the others, because it does not have unequal numbers of the two parental genotypes and therefore does not qualify for consideration in a chapter on conversion. Yet all the types of aberrant segregation are associated with crossing-over and it is arguable that the simplest approach is to treat them all together.

Some of the difficulty for students in understanding what is known about genetic recombination derives from complexities inseparable from the organisms investigated. Thus, in lambda not only are there two recombination

systems specific to it, but the host recombination pathways also act on the phage. Furthermore, the method by which lambda packages its DNA in the head of the virus probably leads to a bias in the recombination events shown by the progeny. With T4 the complexity of the experimental results arises from the extraordinary genetic system that prevails in this phage, which has linear genomes that are circularly permuted with respect to one another. Although a concise account is given of the relevant results, it would be an advantage to students if they already had some knowledge of recombination studies with *E. coli* and these phages.

The book is addressed to graduate students and final-year undergraduates, but I fear that many will find it difficult reading. The problems at the end of each chapter will no doubt help towards a better understanding, though some of these will certainly present a severe challenge for students. Fortunately, the solutions are given later in the book. The clarity of thought that the book constantly demands is helped by a profusion of unnumbered small diagrams which form an integral part of the text. In some of the diagrams the differences in line thickness that indicate the parentage are easily overlooked, and there are no legends to draw one's attention to them. The author's entertaining, often colloquial, style helps to enliven the book.

Errors are few, but Figures 8-1 and 8-2 have been transposed and an incorrect reference is given in this chapter for data on

Sordaria asci with 5:3 ratios for spore colour. In the previous chapter Rossignol and Haedens' work is discussed without reference to them. There is a useful glossary but Stahl's new term, 'splatch', is not included though freely used in the text. It is surprising in a book on genetic recombination to find no reference to the important contributions to the subject by Benbow *et al.*, Ephrussi-Taylor *et al.*, Spatz and Trautner or Tiraby *et al.*, even though their work relates to organisms other than those with which the book is primarily concerned.

My chief criticism of Stahl's book is that it does not give a balanced view of the current state of knowledge of recombination in eukaryotes. Hypotheses involving fixed pairing segments, such as his so-called "sex-circle" model, are discussed without drawing adequate attention to the conflict they present with a considerable body of data. He lists eleven features of the recombination data for *Saccharomyces cerevisiae* and implies that these results are so different from those found with other fungi as to demand a quite different explanation. But most of the results with yeast have also been found in other fungi; and to me the similarities are much more impressive than the differences. He supports the idea of break-and-copy by reference to work on phage $\phi 1$ without acknowledging that the authors subsequently abandoned this hypothesis for a more conventional one depending on heteroduplex DNA. And why suppose that

in fungi two crossovers (reciprocal exchanges) may occur in very close proximity in the same two chromatids when there is no evidence for this? Stahl repeatedly raises the subject of interference between crossovers, even to the final sentence of the text, where he says "... we would all feel better if a good molecular explanation for the facts of interference were neatly wedded to some of those models", referring to those he has been discussing. But he seems to overlook the very great distances, in molecular terms, over which interference often acts, and the lack of it across the centromere. It seems likely that interference is a phenomenon superimposed on the basic crossover mechanism rather than a primary feature of it. When discussing Holliday's recombination model in Chapter 8, Stahl argues that interference should lead to an excess of recombinant over parental flanking marker genotypes in association with conversion, but Holliday himself (*Phil. Trans. R. Soc. B277*, 367; 1977) draws the opposite conclusion.

Despite these criticisms, Dr Stahl is to be congratulated for producing such a challenging and thought-provoking, if controversial, work. It should draw attention to the many unsolved problems in this field and provide a stimulus for further research. □

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Illustrated black holes

R.L. Znajek

Black Holes. By W. Sullivan. Pp.303. (Anchor Press/Doubleday: Garden City, New York, 1979.) \$17.95.

SULLIVAN'S *Black Holes* begins with a bang. Literally. The first chapter is about the Tunguska event — the great Siberian explosion of 1908. This is tenuously linked to the title by a theory that the explosion was caused by a small black hole falling through the atmosphere (Jackson and Ryan, *Nature* **245**, 88–89; 1973). Like all good theories it made a prediction: the hole passed through the Earth and emerged over the North Atlantic Ocean, causing a similar explosion as it left. This should have caused atmospheric vibrations comparable to those detected from the original event. It turned out that no such vibrations were detected from the North Atlantic (Beasley and Tinsley, *Nature* **250**, 555–556; 1974), and so the theory now seems untenable. There is of course no shortage of other ideas, ranging from the exotic (an

exploding nuclear-powered spacecraft) to the quite plausible (a small comet or meteorite). Sullivan tells us about all of them, and this gets him into digressions about anti-matter and carbon-14 dating. We are treated to maps, eyewitness reports and even photographs of a man who was blown off his porch by the blast. It is mostly quite interesting. Like the rest of the book, it has little to do with black holes. But the title presumably sells.

Black Holes is in fact an account of some of the more dramatic goings-on in modern astronomy. It deals with white dwarfs, pulsars, X-ray sources, quasars and cosmology, as well as with things that might have exploded over central Siberia. Sullivan, being Science Editor of the *New York Times*, writes in an appropriately journalistic 'who-did-what' manner. He emphasizes observations and is short on theory. The trouble is that black holes are inherently theoretical objects. They cannot, by definition, be seen or photographed. Events happening near a black hole can be observed, and the hole's presence deduced theoretically. A book about black holes should certainly explain why many astronomers think they exist, and this one does. It should also make a serious attempt to explain what they are,

and this means more than just stating that gravity becomes so strong that not even light can escape.

I am not asking for a layman's guide to tensor calculus and the Einstein equations. I am suggesting that one can give a precise but non-mathematical account of relevant aspects of special and general relativity, such as light cones and frames of reference. By showing how light cones are tilted at the surface of a black hole, one can demonstrate why the surface is an 'event horizon' and why leaving it is precisely equivalent to travelling faster than light. (A spaceship that can travel faster than light can get out of a black hole. Scientists depicted in the recent film *The Black Hole* seem surprisingly ignorant of this fact.) Laymen may not understand mathematics, but most of them can read maps, and so any popular account of black holes should include plenty of space-time diagrams, which are just maps showing the location of events in space and time. The best example of what I have in mind is Kaufmann's book *The Cosmic Frontiers of General Relativity* (Little, Brown: Boston, 1977), which might almost be accused of being over-illustrated. Sullivan's book is crammed with photographs of astronomers, telescopes, rockets and galaxies, but I cannot

find a single space-time diagram in it.

Having made my complaints, I must confess I quite liked the book. All that is wrong with it is the market it is aimed at. It is basically an anecdotal history, and as such is more likely to interest astronomers than the general public. It provides a personal dimension which is normally absent in original papers and review articles. Sullivan records the comments of scientists in the process of discovering, debating or just feeling confused. The result is somewhat chaotic, but that is what

science is often like. Sullivan himself describes his book as "an ongoing scientific mystery story". He provides an excellent set of "Selected References", the main merit of which is that they are not selected. Most of them are very inappropriate for laymen, but they are precisely what is required by astronomers wishing to get to the science behind the anecdotes. The book would be improved by the removal of some of the weaker attempts at popularization, such as the summing-up of diffraction as "light plus

light equals darkness". There are a few inaccuracies, like the statement that in an accretion disk "gas would spiral in more rapidly were it not for friction". (Gas would not spiral in at all were it not for friction.) I can nevertheless recommend this book for the history section of any astronomical library. I would recommend Kaufmann's book to any layman who wants to know about black holes. □

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Thames revival

R.J. Wootton

The Tidal Thames. By Alwyne Wheeler. Pp.228. (Routledge and Kegan Paul: London and Boston, 1979.) £8.95.

THE tidal Thames runs from Teddington Lock, west of London, eastwards to the North Sea. Bisecting the capital city, this length of the river has streamed through British history, providing a means of transport for men and their commerce. Until the nineteenth century, it also supported flourishing fisheries which supplied London with such esteemed food fish as salmon, smelts, eels and whitebait. But thereafter a combination of events destroyed these fisheries and left a long stretch of the tidal river apparently lifeless. Only in the past two decades has this degradation of the river been reversed, and now fish can once again be found throughout the tidal Thames. Alwyne Wheeler, an ichthyologist at the British Museum (Natural History), has played an important role in recording the return of fish to the Thames. His book tells of the death and revival of the river as a habitat fit for fishes.



In the first half of the book, Wheeler describes the decline in quality of the river and its effects on the fisheries. Industrialization and population growth were the major factors. Pound locks were constructed to improve navigation on the river, but these hindered the movements of migratory fish and made their capture easier. Gas works and other industries poured their toxic by-products into the river. The primitive methods for the disposal of human excrement, cess pits with the night soil spread on agricultural land, became inadequate so that disposal

through sewers was introduced, a development encouraged by recurrent outbreaks of cholera. At first the sewers discharged directly into the river in London, but this led to such gross pollution that on an infamous occasion in 1858, "the year of the great stink", committee meetings in the Houses of Parliament were suspended because of the stench. A great Victorian engineering scheme was initiated, and in a few years 84 miles of sewers had been built which carried London's wastes eastwards to empty into the river downstream of central London. Sadly, the site for the outfalls was miscalculated, and the sewage was trapped in the water mass by tidal movements. This area of the river, receiving most of London's excrement, became so polluted it was claimed that suicides died of poisoning before they could drown themselves. If the outfalls had been a few miles downstream it is possible that the Thames would never have become lifeless. Although the situation had improved by the beginning of the twentieth century, two world wars, and the continued growth of population in the London area without adequate provision for sewage treatment, meant that by the 1950s there were no established fish populations in the Thames over a distance of 69 km. Most of this stretch had extremely low dissolved oxygen concentrations, the consequence of the massive organic pollution.

This intolerable state was recognized and major improvements in sewage treatment started. The effect of these improvements on the fish fauna was monitored in an original way. Wheeler arranged for the collection of fish caught on the filter screens of electricity generating stations which extracted cooling water from the Thames. These collections yielded a clear picture of the recovery of the fish fauna, as both the number of species and the number of individuals increased. This picture of a cleaner river was emphasized dramatically when salmon were recorded in the Thames for the first time in 140 years.

The second half of the book is a detailed description of the results of Wheeler's and other collections made since 1967, in which 72 species were recorded. This section makes difficult reading. Much of the

quantitative data could have been relegated to an appendix for, as Wheeler himself notes, the method of collection provided a qualitative rather than quantitative picture of the recovery of the fish fauna. Yet some of the detail is inadequate: for some species inadequate morphological detail is given; and authorities are not given for the specific names while some common names are used inconsistently.



Finally, Wheeler notes that a cleaner river is more sensitive to environmental disturbances which would have been insignificant when the river was lifeless. Potential hazards include the effects of large power stations on water temperature and hence dissolved oxygen, the discharge of raw sewage during storm overflow, and the effects of flood prevention schemes. Although it is unlikely that commercial fishing will again flourish in the tidal Thames, the aesthetic and recreational benefits provided by established fish populations are incalculable. Wheeler's book illustrates how effective public spending, which is now under attack, can be in a civilized, industrialized state.

Although there are a few typographical errors and mistakes of chronology, the book is reasonably priced. Many of the illustrations are taken from Yarrell's *British Fishes* (1859), which links Wheeler's book with that of a naturalist familiar with the Thames before it died of pollution. □

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Surprise!

J.S. Bell

Surprises in Theoretical Physics. By R. Peierls. Pp.166. (Princeton University Press: Princeton, 1979.) Hardback \$19, £8.40; paperback \$5, £2.10.

THIS is a fascinating book. About thirty situations are presented, scattered widely over theoretical physics. The surprise is sometimes that the obvious answer is wrong, sometimes that the devastating criticism of the obvious answer is wrong, sometimes that a very great man has gone wrong, and so on. Some of the surprises are pleasant, as with the validity of the nuclear shell model, or with the simplicity of Landau's diamagnetism. Some of the surprises are unpleasant, as with the non-existence of the Bogolyubov expansion for the diffusion coefficient, or even with the difficulty of estimating the adequacy of the JWKB method in barrier penetration. Some of the distinguished men who slipped up were very distinguished indeed. Debye was quite wrong about thermal conductivity in non-metals, and the "reasonable agreement with experiment fortuitous". Heisenberg was wrong about the resolving power of his γ -ray microscope, and was corrected by Bohr. Einstein forgot his own gravitational redshift in an attack on the time-energy uncertainty relation, and he also was corrected by Bohr. But Peierls himself remains surprised that Bohm and Aharonov have successfully attacked a version of this "fourth uncertainty relation". He conjectures that the interaction they invoke "for some reason cannot be permitted in quantum mechanics".

Sometimes the important reference is to early work of Peierls himself. It is the 'Umklapp' process of his 1929 thesis which resolves the difficulty with Debye's thermal conductivity. Sometimes the surprise has been generated by colleagues in his own department. But the choice is catholic, and indeed the two longest sections in the book are concerned with two perennial surprises — irreversibility in statistical mechanics, and the problem of interpreting quantum mechanics.

It seems to me that the non-technical account of macroscopic irreversibility, contrasted with microscopic reversibility, brings out admirably the essential point often concealed in lengthier and more mathematical accounts. That is to say that the situation is intelligible when we suppose boundary conditions to be imposed in the past, rather than the future, and with no great care — or at least without the fantastic and conspiratorial care that could have ensured the exceptional decrease of entropy rather than the normal increase.

As regards the problems of quantum mechanics, Peierls begins with an account

of what went wrong with the celebrated von Neumann theorem on the impossibility of deterministic hidden variables. He continues with a nicely ironical account of subsequent work centred on the notion of 'locality'. Then he takes up the infamous 'reduction of the wave packet'. To my dismay he regards it as "clear that the significance of the state function is to represent our state of knowledge of the system". But he goes on to ask "whose knowledge . . .", and is carried down into the depths. He finally conjectures that the Schrödinger equation does not apply to conscious organisms and (if I interpret correctly) that it is in the presence of such organisms that linearity fails and wave function collapse occurs. It seems to me that the reduction is then dynamical rather than actuarial. It is not at all to be equated with the mere adjustment of odds appropriate when a candidate for life insurance is seen to be over one hundred.

The bulk of the book is occupied with quantum and statistical mechanics. But finally there is one relativity problem. It is the old question of radiation — or non-radiation — from a uniformly accelerated charge. Here (surprise) there is something quite new, from unpublished work of Boulware. He has worked out carefully (for the first time, it seems) how it looks in the accelerated system in which the charge is at rest. I would have liked to have seen my favourite relativity surprise included in this book. I will yield here to the temptation to describe it (A. Evett, *Am. J. Phys.* **40**, 1170; 1972). Two identical spaceships,

identically programmed, are initially at rest in some inertial system S, one of them 100 metres behind the other. At a given moment (in S) both motors start and off they go. With identical acceleration programmes they remain, of course, always 100 metres displaced from one another (in S). The ships are initially connected by a fragile (but incombustible!) thread. This thread would like to Fitzgerald — contract as the ships speed up, but as they do not come closer (in S) the thread cannot contract — so it breaks. Or does it? My experiences with this puzzle have convinced me that most relativity courses seriously damage the minds of most students.

There are no surprises here from elementary particle theory. Is that, then, only a dull plodding sort of subject? I think not. For example, there was the big surprise of a renormalizable theory of weak interactions. And I think also of the very beautiful surprise of the 't Hooft-Poylakoff magnetic monopole. And surely there are many others.

But let us be grateful for what we do find in this excellent book. One of the nicest surprises in it is the elegant simplicity with which nearly all of these topics are presented. The essays are mostly accessible to undergraduates with a first course in quantum mechanics, and to graduates who have not forgotten. □

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Memorial to the Ghetto

John Rivers

Hunger Disease. Edited by Myron Winick. Pp.261. (Wiley: New York and Chichester, UK, 1979.) \$19.80, £10.60.

THIS is an important book. It was conceived early in 1942 by Dr Israel Milejowski who, as chief of public health in the Warsaw Ghetto, was sure that death from starvation and disease awaited its inhabitants. Certain of this fate, and powerless to prevent it, he recruited 27 colleagues and persuaded them to undertake a scientific study of the effects of hunger that they were observing in the Ghetto. Such a study, he hoped, would be, at least, a memorial to the dead.

They had just five months to build that memorial. On 22 July 1942 the German authorities began to liquidate the Ghetto and its inhabitants. Milejowski records that their already meagre hospital and laboratory facilities were destroyed, and, with mass deportations occurring around them, the scientists wrote up their work

and prepared the manuscript that was smuggled out of the Ghetto and hidden.

It was a remarkable feat, but ironically it almost seemed as if their efforts were in vain. Though the manuscript survived the war, only limited editions were published in Polish and French, and though the title *Maladie de Famine* was often referred to in the nutrition literature, the document itself was largely unread by nutritionists.

In rescuing it from obscurity, and producing not only an English translation, but a detailed appreciation of the modern significance of the work, Professor Winick has ensured that the memorial Milejowski intended has at last been built.

But the book must not be viewed as a purely historical document, and Professor Winick has wisely treated it as a work of current scientific merit. Of course it is a strange scientific document, exploring theories and using techniques of 40 years ago. But outmoded or not it is still of current interest, because, with the exception of Keys' *Biology of Human Starvation* (University of Minnesota Press, 1950), it is the only study in the field. Though by its brevity it is less authoritative than Keys' work, it ranks alongside it as a scientific document because it deals with some different aspects of starvation, and

Severe marasmus and death in the Warsaw Ghetto.



because, unlike Keys' experimental study, it deals with people starving to death.

I am sure that many of the results the authors obtained will still be of scientific interest.

For example, the chapter by Fliederbaum *et al.* on metabolic changes records that in severely malnourished patients BMR was 30–40% below normal and was not stimulated by protein feeding, but increased by 20–50% when sugar was fed. Or again, Fajgenblat's brief report on ocular changes in starvation, or Apfelbaum-Kiwalski's report on the pathophysiology of the circulatory system in starvation, will be read with interest by all workers in the field. All will, like me, be saddened by the brevity of the reports: in reading the book I often found myself wishing that all the authors had reported their raw data, or referenced the exact methods by which they worked out complex indices (like the degree of normality of BMR) but, given the conditions in which they worked, one can only be grateful that anything at all has survived.

That which has survived will be accorded a place of honour amongst many analogous nutritional studies on the pathophysiology of protein-energy malnutrition in children that have been produced since the Second World War.

However there will remain one crucial difference between this book and many other postwar studies on the biochemistry of malnutrition. Most scientists studying malnutrition since the war have done so because they believed that the causes and cures of malnutrition should be sought at

the physiological level. The authors of this book had no such illusion. The cause of the malnutrition they describe is to them clear: it is the result of a systematic policy which, in isolating the Jews from the economic life of the nation, sentenced them to death. It was a policy which allowed a Jew only 800 kcal per day, under half of that allowed by the Germans even for people who did no work worth mentioning. It was in response to this policy that the authors undertook their study, not because they believed they would find a scientific cure for the Hunger Disease that this policy induced but because all they could do as scientists was to create a memorial to the dead, by their contribution to scientific knowledge.

Is there in this a lesson for our times? In the 40 years since the Ghetto was destroyed, immense scientific effort has been put into studies of the nutritional and metabolic aspects of protein-energy malnutrition in children without doing anything to reduce the prevalence of the disease. Perhaps the heroic efforts of Milejowski and his colleagues should cause us now to focus more clearly on the cause of the disease and step outside a narrow scientific paradigm to seek a cure.

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O polish'd perturbation

A.J. Meadows

Planets X and Pluto. By William G. Hoyt. Pp.302. (University of Arizona Press: Tucson, Arizona, 1980.) Hardback \$17.50; paperback \$9.50.

A YEAR from now we will be celebrating the two-hundredth anniversary of the discovery of the planet Uranus by William Herschel. Compared with, say, the Einstein centenary which occurred recently this might seem to be an event of limited interest. In terms of its contemporary impact, however, Herschel's discovery attracted as much public attention as the General Theory of Relativity did in this century. For the first time since prehistory, the scope of the Solar System had been enlarged. The natural reaction of astronomers was to look for other, as yet undiscovered, planets — a hunt which lasted for a century and a half.

Mr Hoyt's book is primarily concerned with the investigations that led to the last of these planetary discoveries — the detection of Pluto. But it is logical that he should introduce this search with a detailed discussion of the events surrounding the discovery of earlier planets before turning to Pluto itself. His account is readable, but highly detailed. Like his earlier book,

Lowell and Mars, the narrative is partly based on material in the Lowell archives. Like the earlier book, too, it is intended as a partial history of the Lowell Observatory, and so covers matters other than planets. But it is the search for Planet X — as the supposed planet beyond Neptune was labelled — that dominates the story.

The main outlines of the hunt for new planets is fairly familiar, at least up to the discovery of Neptune. But this earlier history — and especially the problems facing the theoretical prediction of Neptune's position in the sky — provides a fascinating parallel with the subsequent search for Planet X. Neptune was sought because the newly discovered Uranus stubbornly refused to follow its predicted path. The postulate of an outer planet, gravitationally perturbing Uranus, became an increasingly attractive possibility. Ultimately, two theoretical astronomers, Leverrier and Adams, independently predicted a position for this supposed planet. Their results were in good agreement with each other, and Neptune was, indeed, picked up close to the predicted point. So far, this was a major success story. But it rapidly became evident that some of the assumptions made by both Leverrier and Adams in determining the new planet's position were far removed from reality. How, then, had they managed to pinpoint its place so accurately? It was very quickly suggested that their result was purely a 'happy

accident', a conclusion which was equally quickly denied. The controversy continues today (it is, perhaps, a slight defect in this book that the subsequent analyses of the problem are not fully covered); but a modern consensus would give Leverrier and Adams the benefit of the doubt.

The search for Planet X possessed one immediate difference from the hunt for Neptune. Like the latter it was based on an examination of residuals, otherwise unaccounted for, in the orbits of the inner planets (in this case, Uranus and Neptune). The residuals now, however, were much smaller than those that had originally attracted attention to the existence of Neptune. It was less that these residuals forced a search for a new planet than that the desire to find a new planet motivated the investigation of the residuals.

The most detailed, though not the only, attack on the problem of Planet X was by Percival Lowell, and much of the book revolves round his activities. Lowell's final predictions appeared in 1915 — only a year before his death — but the astronomers at Lowell Observatory had already started on a photographic search for the supposed planet in 1905. After Lowell's death the search lapsed, only to be resumed at the end of the 1920s. It was then undertaken by Tombaugh, a new recruit to the observatory staff, and, early in 1930, he discovered Pluto close to the point indicated by Lowell's calculations.

This sounds like a repeat of the

successful hunt for Neptune. The similarity extends even further, for one of Lowell's contemporaries, W. H. Pickering, had predicted the existence of a planet at about the same place as that suggested by Lowell. (The parallel with Leverrier and Adams cannot be pushed too far, however, for Pickering enthusiastically predicted the existence of several planets at various distances from the Sun.) There was one major difficulty: both Lowell and Pickering supposed in their calculations that Planet X must be a reasonably massive body. Pluto, when found, proved to be fainter, and so presumably smaller, than expected. Was it really the predicted planet? The arguments started once again:

rather unproductively this time, because no mass was available for Pluto. The climax both of this dispute and of the book has actually only just occurred: for, in 1978, a Plutonian satellite was finally discovered. The consequences are remarkable — to quote Mr. Hoyt:

"The discovery of a Plutonian satellite [named Charon], of course, permitted the first direct and reasonably accurate determination of Pluto's mass since the planet's discovery forty-eight years ago . . . Pluto is thus a very, very small planet indeed, only about 20 percent as massive as the earth's moon, and with a probable diameter of only 1500 miles (2420 km), about two-thirds that of the earth's moon.

"The discovery of Charon, and the consequent determination of Pluto's very small mass, made it certain that Pluto could not be Percival Lowell's predicted Planet X, and thus the long controversy over this question ended."

The result is stated firmly; but it leads to the very odd conclusion that the positions of Pluto (certainly) and Neptune (possibly) were correctly predicted by accident. The Einstein centenary reminded us that God does not play dice. Perhaps the Uranus bicentenary should assure us that He does.

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New astronomies

R.J. Tayler

A Source Book in Astronomy and Astrophysics, 1900-1975. Edited by K.R. Lang and O. Gingerich. Pp.922. (Harvard University Press: Cambridge, Massachusetts, and London, UK, 1980.)\$50.

THIS book provides an attractive invitation to review the great increase in our knowledge of the Universe over the present century. In 1900, most astronomers believed that the Milky Way system was the whole Universe and that the Sun was near its centre. The existence of interstellar matter was suspected but not established and it was not known that interstellar dust absorbed starlight and produced a seriously biased view of the Universe. Although spectroscopy had shown that stars contain the same chemical elements as the Earth, a quantitative discussion was impossible because the structure of atoms and the origin of spectral lines were not understood. A study of stellar structure had started but its further development required more knowledge of atomic physics and of nuclear structure, leading to an explanation of the source of stellar energy.

For the first two-thirds of the period under review, observational advances were restricted to optical astronomy. The Universe was seen to be an immense expanding system of galaxies and the structure of our Galaxy and its constituent stars and gas clouds was studied in great detail. Towards the end of this great period of optical astronomy, many astronomers believed that all the important components of the Universe had been discovered. This view has been transformed in the past twenty-five years by the development of new astronomies — radio, infrared, ultraviolet, X-ray and γ -ray — studying the 'invisible Universe', and by the discovery, for example, of quasars, pulsars, X-ray binaries, interstellar neutral hydrogen and molecules and the cosmic microwave radiation. Theoreticians have

used developments in physics, such as quantum theory and general relativity, to explain the properties of objects already known and to predict the existence of new types of object.

Drs Lang and Gingerich have selected about 150 key papers to trace the increase in our knowledge. In some cases the papers are printed in full and in others the less essential parts of the discussion have been paraphrased. In every instance, they have introduced their selection by some general remarks on the development of the subject; this enables them to give due credit for key work which they could not include in the anthology. I find this a fascinating collection of papers and I am impressed by

the quality of the editorial work. In reading the book, I have learnt many things which I had not previously known and others which I had forgotten. The book should certainly be widely available to both young and old students of astronomy. It is inevitable that not all astronomers will agree with the choice of key papers. In general, I did find what I expected but I was surprised that, although the work of Chandrasekhar is widely quoted, not one of his papers is included and I would have included Parker's paper on the solar wind.

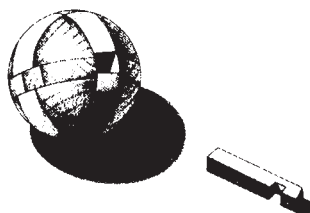
R.J. Tayler is Professor of Astronomy and Director of the Astronomy Centre at the University of Sussex, Brighton, UK.

CONTINUING CHANGE IN SCIENTIFIC THOUGHT

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- DOLLARD, J.D. and FRIEDMAN, C.N. *Product Integration with Applications to Differential Equations*. Encyclopedia of Mathematics and its Applications Volume 10. Pp.xvii + 253. ISBN-0-201-13509-4. (Massachusetts, London, Amsterdam, Ontario, Sydney, Tokyo: Addison-Wesley, 1979.) \$24.50.
- MARSHALL, A.W. and OLKIN, I. *Inequalities: Theory of Majorization and Its Applications*. Mathematics in Science and Engineering Volume 143. Pp.xx + 569. ISBN-0-12-473750-1. (New York, London, Toronto, Sydney, San Francisco: Academic Press, 1979.) \$49.50.

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- ROCHESTER, S. and MARTIN, J.R. Crazy Talk. A Study of the Discourse of Schizophrenic Speakers. Cognition and Language: A Series in Psycholinguistics. Pp.xii + 229. ISBN-0-306-40236-X. (New York and London: Plenum, 1979.) \$25.00.
- SIMMEL, E.C. (ed.) Early Experiences and Early Behaviour. Implications for Social Development. Pp.viii + 217. ISBN-0-12-644080-8. (New York, London, Toronto, Sydney, San Francisco: Academic, 1979.) \$18.00.
- VICKERS, D. Decision Processes in Visual Perception. Academic Press Series in Cognition and Perception. Pp.vi + 406. ISBN-0-12-721550-6. (New York, San Francisco, London: Academic, 1979.) £21.60, \$50.00.
- WISHNIE, H.A. and NEVIS-OLESEN, J. (ed.) Working with the Impulsive Person. Pp.viii + 184. ISBN-0-306-40184-3. (New York and London: Plenum, 1979.) \$19.50.

History

- BICKEL, L. The Deadly Element. The Story of Uranium. Pp.312. ISBN-0-333-24158-4. (London and Basingstoke: Macmillan, 1980.) £7.95.
- BRINTNALL, D.E. Revolt Against the Dead. The Modernization of a Mayan Community in the Highlands of Guatemala. G & B Library of Anthropology. Pp.xxiii + 199. ISBN-0-677-05170-0. (New York, London, Paris: Gordon and Breach, 1979.) £10.00.
- BROWN, S.C. Benjamin Thompson, Count Rumford. Pp.ix + 361. ISBN-0-262-02138-2. (Cambridge, Massachusetts and London: MIT, 1979.) £12.40.
- CAJORI, F. A History of Mathematics. Third Edition. Pp.x + 524. ISBN-0-8284-0303-1. (New York: Chelsea Publishing Company, 1980.) \$18.50.
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General

- BLACKMUR, R.P. The Revival. Pp.17. (New York and London: Harcourt Brace Jovanovich, 1979.)
- BOND, G., JOHNSON, W. and WALKER, S.S. (ed.) African Christianity patterns of religious continuity. Studies in Anthropology. Pp.xvi + 175. ISBN-0-12-113450-4. (New York, London, Toronto, Sydney, San Francisco: Academic, 1979.) \$19.50.
- BROWN, B.J. Disaster Preparedness and the United Nations. Advance Planning for Disaster Relief. Pergamon Policy Studies on Socio-Economic Development. Pp.xviii + 147. ISBN-0-08-022486-5-H. (New York, Oxford, Toronto, Sydney, Frankfurt, Paris: Pergamon, 1979.) £8.00.
- MEDAWAR, P.B. Advice to a young Scientist. Alfred P. Sloan Foundation Series. Pp.xv + 109. ISBN-0-06-013029-6 (U.S.A. and Canada) ISBN-0-06-337006-9 (except U.S.A. and Canada). (New York, Cambridge, Hagerstown, Philadelphia, San Francisco, London, Mexico City, Sao Paulo, Sydney: Harper & Row, 1980.) £4.95.
- MOSLEY, R.K. Westminster Workshop: A Student's Guide to British Government. Pp.ix + 217. ISBN-0-08-0206305-2. (Oxford, New York, Paris: Pergamon Press, 1979.) \$8.00, £4.00.
- O'LEARY, De Lacy. How Greek Science Passed to the Arabs. Pp.vi + 196. ISBN-0-7100-1903-3. (Henley-on-Thames: Routledge & Kegan Paul, 1979.) £5.95.
- RUNTE, Alfred. National Parks: The American Experience. Pp.xv + 240. ISBN-0-8032-3852-5. (Lincoln and London: University of Nebraska Press, 1979.)

ANNOUNCEMENTS

Appointments

The following have been elected Fellows of the Royal Society: **Professor Muhammad Akhtar**, (University of Southampton); **Dr Zbigniew Stanislaw Basinski**, (National Research Council of Canada); **Professor Richard Evelyn Donahue Bishop**, (University College London); **Professor Donald Charlton Bradley**, (Queen Mary College, University of London); **Dr John Burns Brooksby**, CBE, (Formerly, Animal Virus Research Institute, Pirbright, Surrey); **Professor Paul Moritz Cohn**, (Bedford College, University of London); **Dr Henry Ellis Daniels**, (University of Cambridge); **Professor James Philip Elliott**, (University of Sussex); **Professor Douglas Hugh Everett**, MBE, (University of Bristol); **Dr Frederick Gerard Friedlander**, (University of Cambridge); **Professor Brian Edgar Scourse Gunning**, (Australian National University); **Professor William Donald Hamilton**, (University of Michigan); **Dr Marshall Davidson Hatch**, (CSIRO, Australia); **Dr Leslie Lars Iversen**: MRC Neurochemical Pharmacology Unit, Cambridge); **Professor Kenneth Henderson Jack**, (University of Newcastle-upon-Tyne); **Professor William Fleming Hoggan Jarrett** (University of Glasgow); **Professor Niels Kaj Jerne**, (Basel Institute of Immunology); **Professor Alan Roy Katritzky**, (University of East Anglia); **Professor Thomas Walter Bannerman Kibble**, (Imperial College of Science and Technology, University of London); **Dr Anthony Seymour Laughton**, (Institute of Oceanographic Sciences, Godalming, Surrey); **Dr Richard Maitland Laws**, (British Antarctic Survey, Cambridge); **Professor Anthony James Leggett**, (University of Sussex); **Professor Graham Collingwood Liggins** (University of Auckland Medical School); **Professor Anthony William Linnane** (Monash University, France); **Mr Mohtague Mattinson Pennell**, CBE, (BP Co. Ltd); **Dr György Károly Radda**, (University of Oxford); **Professor Humphrey Peter Rang**, (University College London); **Professor Mark Henry Richmond**, (University of Bristol Medical School); **Professor Derek Harry Roberts**, (Hirst Research Centre, Wembley, Middlesex); **Professor John Arthur Shercliff**, (University of Warwick); **Professor Louis Siminovitch** (University of Toronto, Canada); **Dr Herbert Williams Smith**, (Houghton Poultry Research Station, Hertfordshire); **Professor Walter Eric Spear**, (University of Dundee); **Dr Grenville Turner**, (University of Sheffield); **Professor Lawrence Weiskrantz** (University of Oxford); **Professor Walter Thompson Welford**, (Imperial College of Science and Technology, University of London);

Professor William Henry Wittrick, (University of Birmingham); **Professor Lewis Wolpert**, (Middlesex Hospital Medical School, London); **Mr Alan Marshall Muir Wood** (Sir William Halcrow and Partners, Consulting Engineers).

Dr Paul Broda has been appointed UMIST's first Professor of Applied Molecular Biology from September 1980.

Among those elected to membership of the Royal Irish Academy are: **Patrick Finbarr Duggan**, (Cork Regional Hospital); **Michael Alphonsus Hayes**, (University College, Dublin); **Brian Henderson**, (Dublin University); **Amyan MacFayden**, (New University of Ulster, Coleraine).

Professor John Horlock FRS, vice-chancellor of the University of Salford, will succeed Lord Perry of Walton as Vice-Chancellor of the Open University. He will take up his duties on 1 January 1981.

The following have been appointed to chairs in the University of Bristol: in veterinary medicine, **Dr F.J. Bourne**; and in veterinary surgery, **Dr H. Pearson**.

Dr John J. Lowke has been appointed Chief of CSIRO's Division of Applied Physics.

Dr Richard R. Tavernetti, has been elected to the Board of Directors of The Cyclotron Corporation, Berkeley, California.

Sir Arnold Hall FRS has been elected chancellor of Loughborough University of Technology in succession to Lord Pilkington who is to resign after 14 years in office.

Dr P. C. W. Davies, lecturer in applied mathematics at King's College London, has been appointed to the chair of theoretical physics from 1 October 1980.

Awards

Dr William Shockley will receive the 1980 Medal of Honor from The Institute of Electrical and Electronics Engineers (IEEE), for his co-invention of the transistor for which he shared the Nobel Prize in 1956.

The Royal Society of Victoria (Melbourne, Australia) have awarded the Society's Research Medal for 1979 to **Professor M.J.D. White**.

Dr George Palade (Yale University) has been awarded the 1980 Forteza Memorial gold medal award by the Institute of Cell Research of the Caja de Ahorros of Valencia, Spain.

Dr Yash Pal, director of the Space Applications Centre of the Indian Space Research Organization, has been awarded the sixth Marconi International Fellowship.

The Walter H. Simpsom Fellowship in Photobiology is being offered to doctoral and postdoctoral candidates, having completed at least one year of doctoral course work by September 1980, demonstrating exceptional achievement in that field. Recipient of the Fellowship receives a \$6,000 award for continuing studies on the biological effects of light. Applications to Dr. Philip C. Hughes, Director of Environmental Photobiology, Duro-Test Corporation, North Bergen, New Jersey 07047.

New project

The Management Group of the International Crustal Research Drilling Group are planning two international major crustal drilling investigations during the first half of the decade 1980-89: (1) a 4.5 km hole in Iceland; (2) a series of up to three 1-2 km holes in the Troodos ophiolite massif in Cyprus. Both relate to a major interest of the group in the nature of and physical conditions of formation of oceanic crust. Important specific aspects of both projects are the thermal state of newly formed crust (and thence geothermal energy) and in the conditions of formation of base metal sulphide deposits on the ocean floor. The earliest possible date for commencement of a new drilling project would be 1981. A final decision on which project to pursue first will depend on the results of the investigations to be carried out before May 1980.

Meetings

6 May, **International Symposium on Crop Protection**, Gent (Faculteit van de Landbouwwetenschappen, Coupure Links, 533, B-9000, Gent, Belgium).

7-8 May, **Optical Radiation Measurement**, Teddington (Sira Institute Ltd, South Hill, Chislehurst, Kent, UK).

9 May, **Enkephalins and Endorphins**, Edinburgh (Executive Secretary, The Royal Society of Edinburgh, 22 George St, Edinburgh, UK).

13-15 May, **Post Irradiation Examination**, London (P.J.C. Sumner, CEGB SW Region, Bedminster Down, Bridgewater Road, Bristol, UK).

14 May, **Measurement Standards in Respect of both Ionizing and non-Ionizing Radiation Hazards**, Teddington (British Institute of Radiology, 32 Welbeck St, London W1, UK).

15 May, **AGM of the British Institute of Radiology**, London (British Institute of Radiology, 32 Welbeck St, London W1, UK).

16 May, **Biological Effects of Doses below 2 Gray**, London (British Institute of Radiology, 32 Welbeck St, London W1, UK).

23 May, **Recombinant DNA Technology**, Edinburgh (Executive Secretary, The Royal Society of Edinburgh, 22 George St, Edinburgh, UK).

27-30 May, **Conserving our Resources — The Contribution of Chemical Technology**, Hamburg (Society of Chemical Industry, 14 Belgrave Square, London SW1, UK).

28 May, **Nutrition and Killer Diseases**, London (Dr J. Rose, c/o College of Technology, Feildel St, Blackburn, Lancashire, UK).

28-31 May, **Water and Related Land Resource Systems**, Cleveland, Ohio, (IFAC Symposium '80, c/o AGU, 2000 Florida Ave., N.W., Washington, DC 20009).

2 June, **NMR Tomography: Imaging of Intact Biological Systems by NMR Spectroscopy**, Edinburgh (Executive Secretary, The Royal Society of Edinburgh, 22 George St, Edinburgh, UK).

2-5 June, **7th Ocean Energy Conference**, Washington DC (Gibbs & Cox Inc., 2341 Jefferson Davis Highway, Suite 1020, Century Building, Arlington Virginia 22202).

2-6 June, **Oil Pollution Control**, Ipswich (Miss I. McCann, Institute of Petroleum, 61 New Cavendish St, London W1, UK).

5 June, **Handling Product Information**, London (Aslib, 3 Belgrave Square, London SW1, UK).

6 June, **The Techniques of Reference Work**, London (Aslib, 3 Belgrave Square, London SW1, UK).

9-11 June, **How to get the Best Results from your Liquid Scintillation Counter**, Illinois (Packard Instrument 2200 Warrenville Rd, Downers Grove, Illinois 60515).

9-12 June, **3rd International Congress on the Menopause**, Ostend (The International Menopause Society, 8 av. Don Bosco, 1150 Brussels, Belgium).

10-11 June, **Technology and Challenge of the World's New Fisheries Regime**, London, (Society for Underwater Technology, London SW1, UK).

15-20 June, **Les Systemes de Conversion Thermodynamique de l'Energie Solaire**, Marseille (CNRS, 282 Boulevard Saint-Germain 75007 Paris, France).

16-17 June, **Computer-Produced Indexes**, London (Aslib, 3 Belgrave Square, London SW1, UK).

16-20 June, **Genetic Epistemology and Cognitive Science**, Geneva (University of Geneva, Archives Jean Piaget, 6, rue de Saussure, 1211 Geneva 4, Switzerland).

18 June, **AGM of the Royal Meteorological Society**, London (Royal Meteorological Society, James Glashier House, Grenville Place, Bracknell, Berkshire, UK).

23 June, **Rockstore 80**, Stockholm (Rockstore 80, c/o Stockholm Convention Bureau, Jakobs Torg 3, S-111 52 Stockholm, Sweden).

23-27 July, **The 1980 Paris-Villejuif Immuno-Oncology Week (ICIG, 14 & 16 Ave Paul-Vaillant-Couturier, 94800 Villejuif, France).**

23 June-5 July, **From Nuclei to Particles**, Varenna (Prof. A. Molinari, Istituto di Fisica Teorica dell'Università, Corso M. d'Azeglio, 46 10125 Torino, Italy).

24 June, **The Need for Higher Quality Water**, London (BWETPA, 27 Crendon St, High Wycombe, Bucks, UK).

30 June-2 July, **Oligozoospermia**, L'Aquila (G. Frajese, Clinica Medica V-Policlinico Umberto 1, 00100, Rome, Italy).

25 June, **The Production and Device Applications of Irradiation Induced Defects in Semiconductors**, London (The Institute of Physics, 47 Belgrave Square, London SW1, UK).

30 June-4 July, **Electrophoretic Techniques**, Sussex (Miss L. Hart, Education Dept, Chemical Society, Burlington House, London W1, UK).

30 June-4 July, **International Conference on Evaluation in Science and Technology—Theory and Practice**, Dubrovnik (A. Dulcic, "Ruder Boskovic" Institute, 41001 Zagreb, Croatia, Yugoslavia).

5 July, **Regression of Atherosclerosis in Nonhuman Primates**, Florence (M.R. Malinow, Oregon Regional Primate Research Centre, 505 N.W. 185th Ave, Beaverton, Oregon 97005).

6-11 July, **Radioimmunoassay Workshop**, Guildford (Supraregional Assay Service Course Secretary, Division of Clinical Biochemistry, Dept of Biochemistry, University of Surrey, Stag Hill, Guildford, UK).

7-9 July, **Radio Spectrum Conservation Techniques**, London (The IEE, Savoy Place, London WC2, UK).

9-19 July, **Topics in Ocean Physics**, Varenna (Dr A. R. Osborne, Exxon Production Research Co, PO Box 2189, Houston Texas 77001).

7-11 July, **An Introductory Course in Tropical Hygiene**, London (Ross Institute of Hygiene and Tropical Medicine, Keppel St, (Gower St), London WC1, UK).

14-23 July, **Control of the Growth and Function of Differentiated and Cancer Cells by Intracellular Signals**, Nivelles (NATO Course IRIBHN, School of Medicine, Université Libre de Bruxelles, 2 rue Evers, B-1000 Brussels, Belgium).

19-25 July, **AGM of the American Malacological Union**, Louisville (Dr C. F. E. Roper, Division of Mollusks, National Museum of Natural History, Smithsonian Institution, Washington, DC 20560).

21-23 July, **Soft Ionisation Methods in the Analysis of Biological Substrates**, London (Miss L. Hart, The Chemical Society, Burlington House, Piccadilly, London W1).

21-26 July, **4th International Congress of Immunology**, Paris (Immunology — Congres-Services, 1, rue Jules Lefebvre, F-75009 Paris, France).

21-30 July, **The Biology of Aggression**, Toulouse, (Dr P. F. Brain, Dept of Zoology, University College of Swansea, Singleton Park, Swansea, UK).

21-25 July, **7th International Symposium on Olfaction and Taste, 4th Congress of the European Chemoreception Research Organisation**, Noordwijkerhout (Dr H. Van der Starre, Zoological Laboratory, University of Leiden, 63 Kaiserstraat, 2311 GP Leiden, Holland).

21 July-2 August, **Theory of Fundamental Interactions**, Varenna (Prof. F. Pacanoni, Istituto di Fisica dell'Università, Via Marzola, 8, 35100 Padova, Italy).

4-7 August, **Tryptophan Metabolism: Biochemistry, Pathology and Regulation**, Kyoto (Prof. Ryo Kido, ISTRY-80 KYOTO, Wakayama Medical College, Wakayama 640, Japan).

11-15 August, **Muon Spin Rotation**, Vancouver (μSR2 Conference Secretariat, c/o TRIUMF, University of British Columbia, Vancouver, British Columbia, Canada V6T 2A3).

17-30 August, **New Developments in Membrane Research and Biological Energy Transduction**, Spetsai (Dr G. C. Papageorgiou, Dept of Biology, Nuclear Research Center "Demokritos", Aghia Paraskevi, Attiki, Greece).

18-22 August, **Organic Geochemistry**, New Hampshire (Dr A. M. Cruickshank, Pastore Chemical Laboratory, University of Rhode Island, Kingston, Rhode Island 02881).

24-29 August, **EGS Budapest '80** Geophysical Dept, Eötvös University, H-1083 Budapest, Kun Béla tér 2, Hungary).

27-29 August, **Water Supply and Health**, Noordwijkerhout (Symposium WSH, c/o Klvl, PO Box 30424, 2500 GK The Hague, The Netherlands).

27-30 August, **Chemistry, Pharmacology and Clinical Application of Nitroimidazoles**, Cesenatico (Dr M. D'Angelantonio, via Zanolini 3, 40126 Bologna, Italy).

27 August-6 September, **Radiosensitizers of Hypoxic Cells**, Casenatico (Dr M. D'Angelantonio, via Zanolini 3, 40126 Bologna, Italy).

31 August-5 September, **Immunoassay Workshop — Advanced Course**, Guildford (Courses Secretary, Dept of Biochemistry, University of Surrey, Stag Hill, Guildford, UK).

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31 August-5 September, **Cell Biology**, Berlin (German Convention Service, Congress Organization, Joachimstaler Straße 19, D-1000 Berlin 15, FRG).

31 August-6 September, **Global Impacts of Applied Microbiology**, Lagos (Prof. O.Ogunbi, Giam VI, Dept of Microbiology and Parasitology, Lagos University, P M Bag 12003, Lagos, Nigeria).

1-5 September, **Annual Meeting of the British Association for the Advancement of Science**, Salford (Miss J. H. Dring, BAAS, 23 Saville Row, London W1, UK).

2 September, **Meat in Nutrition and Health**, Colorado Springs (T. J. McDermott, American Meat Science Association, 444 N Michigan Ave, Chicago, Illinois 60611).

2-5 September, **Inborn Errors of Metabolism in Humans**, Interlaken (Mrs S.R. Wyss, Medizinisch-chemisches, Institut der Universität, Buhlstrasse 28, Postfach, CH-3000 Bern 9, Switzerland).

2-5 September, **Medical Librarianship**, Belgrade (Dr L. Sablić, 11000 Beograd, Yugoslavia).

2-5 September, **Medicinal Chemistry**, Torremolinos (Secretariat: VII Int. Symp. on Medicinal Chemistry, 142 Oxford Rd, Cowley, Oxford, UK).

3-5 September, **5th Annual Symposium of the Uranium Institute**, London (Mrs Cherry Wilson, Conference Associates UIS, 34 Stanford Rd, London W8, UK).

5-6 September, **Mammographicum '80**, London (The British Institute of Radiology, 32 Welbeck St, London W1, UK).

5-15 September, **Basic Principles and Methods in Membrane Bioenergetics. Light Energy Transduction by Bacteriorhodopsin**, Szeged (Dr Zs. Dancshazy, Institute of Biophysics, Biological Research Center, Hungarian Academy of Sciences, PO Box 521, 6701 Szeged, Hungary).

8-11 September, **Gas Discharges and Their Applications**, Edinburgh (The Institution of Electrical Engineers, Savoy Place, London WC2, UK).

8-12 September, **World Energy Conference**, Munich (Organizing Committee 11th World Energy Conference 1980, Lindemannstrasse 13, D-4000 Dusseldorf 1, FRG).

9-10 September, **Integration of Metabolism**, Nottingham (The Nutrition Society, Chandos House, 2 Queen Anne St, London W1, UK).

9-11 September, **Stress in Fish** Norwich (Dr A. D. Pickering, Freshwater Biological Association, The Ferry House, Ambleside, Cumbria, UK).

15-19 September, **The Nationwide Provision and Use of Information**, Sheffield (The Aslib/IIS/LA Joint Conference Organiser, 3 Belgrave Square, London SW1, UK).

15-19 September, **Energy Conservation & the Use of Solar & the Renewable Energies in Agriculture, Horticulture & Fishculture**,

London (Rosemary Sweeney, International Seminar Secretariat (SCU), Polytechnic of Central London, 35 Marylebone Rd, London NW1, UK).

16-19 September, **4th European Neuroscience Meeting**, Brighton (Meeting Secretariat, 142-144 Oxford Rd, Crowley, Oxford, UK).

19-23 September, **Workshop on Phytophthora Diseases of Tropical Cultivated Plants**, Kerala (Dr KKN Nambiar, Central Plantation Crops Research Institute, Kasaragod-670 124, Kerala, India).

22-23 September, **Islet-Pancreas Transplantation and Artificial Pancreas**, Athens (Prof. Dr S. Raptis, Diabetes Center and Artificial Pancreas Unit, Dept Clinical Therapeutics, Athens University, PO Box 3127, Athens 606, Greece).

22-23 September, **Human Mutation: Biological and Population Aspects**, Albany (I. H. Porter, Director, Birth Defects Institute, New York State Health Dept, Rockefeller Empire State Plaza Tower, Albany, New York 12237).

22-24 September, **Scanned Image Microscopy**, London (The Rank Prize Funds, 12 Warwick Square, London SW1, UK).

22-25 September, **Smoke Control Energy Noise Road Vehicles**, Bournemouth (National Society for Clean Air, 136 North St, Brighton, UK).

22-26 September, **History of Oceanography**, Woods Hole (J. H. Steele, 3rd International Conference on the History of Oceanography, Woods Hole Oceanographic Institute, Woods Hole, Massachusetts 02543).

25 September, **Effects of Low Temperature on Biological Membranes**, London (Dr G J Morris, Society of Low Temperature Biology, CCAP, 36 Storey's Way, Cambridge, UK).

26 September, **Food Dehydration**, Kansas (D Y C Fung, Dept of Animal Sciences and Industry, Call Hall, Manhattan, Kansas 66506).

1-4 October, **Hormones and Cell Regulation**, Bischenberg (Prof. B. Jeanrenaud, Laboratoire de Recherches Médicales, Université de Genève, Ave de la Roserie, 64, 1205 Genève, Suisse).

5-10 October, **European Workshop on Drug Metabolism**, Zurich (Administrative Secretariat, PO Box 182, CH-4013 Basle, Switzerland).

6-9 October, **10th International Laser Radar Conference**, Maryland (Dr P. T. Woods, Dept of Industry, National Physical Laboratory, Teddington, Middlesex, UK).

7-10 October, **Communications and Transportation: Problems and Prospects for the 80s**, Genoa (Ilc, via Pertinace, Villa Piaggio, 16125 Genoa, Italy).

7-9 October, **Polynuclear Aromatic Hydrocarbons**, Columbus (Dr A. Bjorseth, Battelle's Columbus Laboratories, 505 King Ave, Columbus, Ohio 43201).